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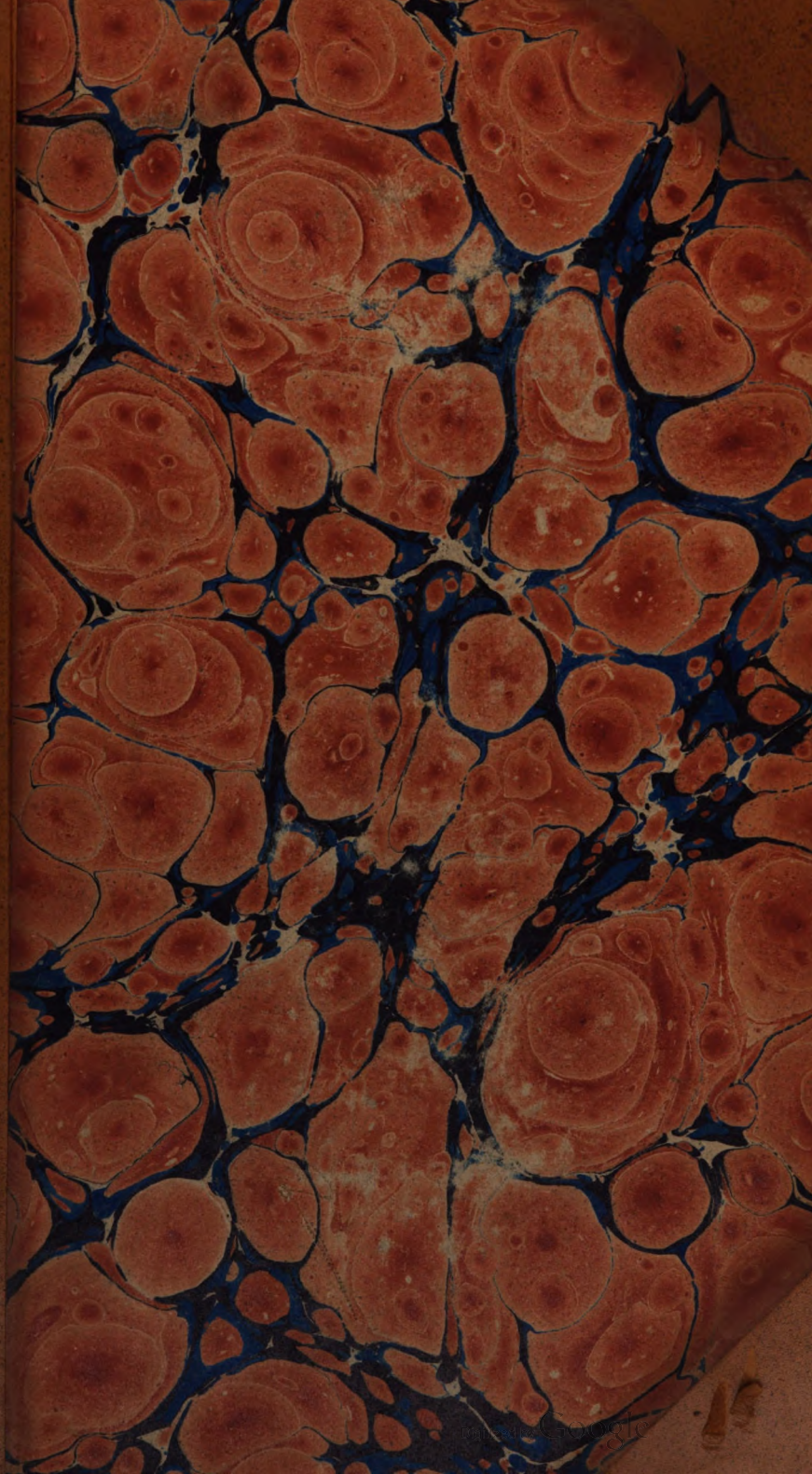
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# THE EDINBURGH- MEDICAL AND SURGICAL JOURNAL.

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 17. Dr ARCHIBALD BILLING on Asiatic Cholera.  
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 19. Dr CHARLES BELL on Cholera Asphyxia.  
 20. Dr EDMUND SKIERS on Diarrhoea, Dysentery, and English and Asiatic Cholera.  
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 2. On the Treatment of Ulcers on the Leg without confinement: with an Inquiry into the best Mode of affecting the permanent cure of Varicose Veins. By HENRY T. CHAPMAN, F.R.C.S.  
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Communications have been received from Dr J. C. STEELE, Dr MAXWELL ADAMS, Dr DUPUIS, and Mr REYNOLDS.

The following publications have been received:—

Memoire sur les faits relatifs a la Revocation de M. Bouillaud des Fonctions de Doyen de la Faculté de Medecine de Paris, et a la Gertun de M. Orfila, Ancien Doyen de la meme Faculté, adressé a l'Assemblée Nationale et A. M. Le Ministre de l'Instruction Publique. Par M. J. Bouillaud, Ancien Deputé Professeur a la Faculté de Medecine de Paris, &c. Paris, 1849. 8vo. Pp. 144.

On the Dependence of Animal Motion on the Law of Gravity. By Henry Wigglesworth, M. B., Scholar in Physiology of the University of London. Part ii. London, 1849. 8vo. Pp. 159-210.

On the Management of the Skin, as a means of Promoting and Preserving Health. By Erasmus Wilson, F. R. S. Third Edition. London, 1849. 12mo. Pp. 238.

Pathological and Practical Observations on Strictures and some other Diseases of the Urinary Organs. By Francis Rynd, Esq., A. M., M. R. I. A., Fellow and Member of Council of the R. C. Surgeons in Ireland, Surgeon to the Meath Hospital and County of Dublin Infirmary, &c. London, 1849. 8vo. Pp. 195.

On Healthy and Diseased Structure, and the true Principles of Treatment for the Cure of Disease, especially Consumption and Scrofula; founded on Microscopical Analysis. By William Addison, M. D., F. R. S., Licentiate of the Royal College of Physicians. London, 1849. 8vo. Pp. 320.

The Medical Remembrancer, or Book of Emergencies: Concisely pointing out the Immediate Remedies to be adopted in the first moments of Danger from Poisoning, Drowning, Apoplexy, Burns, and other Accidents; with the Tests for the Principal Poisons, and other useful Information. By Edward B. L. Shaw, M. R. C. S., and L. A. S., &c. Third Edition. London, 1849. 18mo. Pp. 108.

Parturition, and the Principles and Practice of Obstetrics. By William Tyler Smith, M. D. Lond., Lecturer on Obstetrics in the Hunterian School of Medicine. London, 1849. 12mo. Pp. 395.

Lectures on the Parts concerned in the Operations on the Eye, and on the Structure of the Retina. Delivered at the Royal London Ophthalmic Hospital, Moorfields, June 1847. To which are added, a Paper on the Vitreous Humour, and also a few Cases of Ophthalmic Disease. By William Bowman, F. R. S., F. R. C. Surg., England, &c. &c. London, 1849. 8vo, pp. 143.

Practical Observations on the Prevention, Causes, and Treatment of Curvature of the Spine, with Engravings and Woodcuts Illustrative of the Cases. By Samuel Hare, Surgeon. Third Edition, Revised and Enlarged. London, 1849. 8vo, pp. 245.

A Short Sketch of the Life and Writings of the late Joseph Clarke, Esq., M. D., Vice-President of the Royal Irish Academy, and formerly Master of the Dublin Lying-in Hospital, &c. &c., containing Minute Results of his Private Practice, extending over a series of Forty-Four Years, including Three Thousand Eight Hundred and Seventy-Eight Births. By Robert Collins, M. D., President of the King and Queen's College of Physicians in Ireland, &c. London, 1849. 8vo, pp. 88.

The Calendar of the Queen's College, Birmingham, 1849. Edited by the Dean of the Faculty. Birmingham. 12mo, pp. 160.

Lectures on Medical Missions. Delivered at the instance of the Edinburgh Medical Missionary Society. Edinburgh and London, 1849. 12mo, pp. 320.

A Practical Treatise on Morbus Coxarius, or Hip-Joint Disease, Showing the Advantages to be derived from a System of Mechanical Management for the Prevention and Cure of the Contraction of the Limb. With Cases and Illustrations. By William C. Hugman, M. R. C.

Surgeons, Surgeon to the Verral Institution for the Treatment of Spinal Disease and Distortion, &c. London, 1849. 8vo, pp. 81.

A Practical Treatise on Inflammation of the Uterus and its Appendages, and on Ulceration and Induration of the Neck of the Uterus. By James Henry Bennett, M. D., M. R. C. Physicians, Physician-Accoucheur to the Western General Dispensary, &c. &c. Second Edition. London, 1849. 8vo, pp. 527.

An Account of the Origin, Spread, and Decline of the Epidemic Fevers of Sierra Leone, with Observations on Sir William Pym's Review of the Report on the Climate and Diseases of the African Station. By Alexander Brysson, M. D., R. N. London, 1849. 12mo, pp. 124.

The Misapplication of Anæsthesia in Childbirth, Exemplified by Facts. By G. T. Gream, one of the Medical Officers of the Queen Charlotte's Lying-In Hospital, &c. London, 1849. 8vo, pp.

Effects of Chloroform and of Strong Chloric Ether as Narcotic Agents. By John C. Warren, M. D. Boston, 1849. 12mo, pp. 66.

Memoirs on the Ganglia and Nerves of the Uterus. By Robert Lee, M. D., F. R. S., F. R. C. Physicians, Physician to the British Lying-in Hospital, and Lecturer on Midwifery at St George's Hospital. With Five Engravings. London, 1849. 4to, pp. 36.

Practical Observations on the Diseases of the Uterus. By Robert Lee, M. D., F. R. S., Fellow of the Royal College of Physicians, London, Physician to the British Lying-in Hospital, and Lecturer on Midwifery at St George's Hospital. London, 1849. Folio. Part II. Nine Coloured Plates. (The First Part has not come to hand.)

Principles of Scientific Botany, or Botany as an Inductive Science. By Dr J. M. Schleiden, Extraordinary Professor of Botany in the University of Jena. Translated by Edwin Lankester, M. D., F. R. S., F. L. S., &c., Lecturer on Materia Medica and Botany at the St George's School of Medicine. London, 1849. 8vo, pp. 616.

A Physician's Holiday, or a Month in Switzerland in the Summer of 1848. By John Forbes, M. D., F. R. S. With a Map and Illustrations. London, 1849. Post 8vo, pp. 520.

Essay on the Use of Alcoholic Liquors in Health and Disease. By John Chadwick, M. D., Licentiate of the Royal College of Surgeons, Edinburgh. London, 1849. 12mo, pp. 123.

Portraits of Diseases of the Skin. By Erasmus Wilson, F. R. S. London, 1849. Folio. Fasciculus V. Four Plates. (The four previous Parts have not come to hand.)

Official Documents relative to the Trials of Warburg's Fever Tincture (Tinctura Warburgi) in cases of Intermittent and other Fevers, instituted by order of the Austrian Government. (Translated from the German.) London, 1849. 8vo, pp. 87.

On the Extraction of the Teeth, with an Account of a New and much less Painful Mode of Operating. By Henry Gilbert, M. R. C. S. L. London, 1849. 8vo, pp. 66. With Woodcuts.

The London Medical Gazette, or Journal of Practical Medicine. No. 1111. New Series, 203, March 16, 1849, to 1123. New Series, 215. Friday, June 8, 1849.

The British and Foreign Medico-Chirurgical Review, or Quarterly Journal of Practical Medicine and Surgery. No. vi. April 1849.

London Journal of Medicine. A Monthly Record of the Medical Sciences. No. iv. April. No. v. May. No. vi. June. 1849.

The Dublin Quarterly Journal of Medical Science, No. xiv. May 1849. Dublin.

Weekly Tables of Mortality for the Metropolis. No. xi. 17th March 1849, to No. xxiii. 9th June 1849. London. Annual Series, x.

Eighth and Ninth Annual Reports of the Registrar-General of Births, Deaths, and Marriages, in England. 8vo, pp. 366 and 250. London, 1849.

The American Journal of the Medical Sciences. Published Quarterly. Edited by Isaac Hays, M. D. &c. No. xxxiv. New Series, April 1849. Philadelphia, London, and Paris:

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The following Publications have been received :—

A Dictionary of Practical Medicine; comprising General Pathology, the Nature and Treatment of Diseases, Morbid Structures, and the Disorders especially incidental to Climates, to the Sex, and to the different Epochs of Life, &c. &c. By James Copland, M. D., F. R. S., F. R. Coll. of Physicians, &c., London. Part XIV., being Part V. of Vol. III. Rabies—Scirrhus Tumours. London, 1849. 8vo, pp. 577 to 720.

The Treatment of Rheumatic Diseases by Lemon Juice. With Illustrative Cases from Hospital Practice. By G. Owen Rees, M. D., F. R. S., F. R. C. Physicians, Assistant Physician to, and Lecturer on Materia Medica at, Guy's Hospital, &c. London, 1849. 8vo, pp. 38.

Fruits and Farinacea the Proper Food of Man; being an attempt to prove, from History, Anatomy, Physiology, and Chemistry, that the Original, Natural, and best Diet of Man is derived from the Vegetable Kingdom. By John Smith. Second Edition. London, 1849. 12mo, pp. 341.

On Gout; its History, its Causes, and its Cure. By William Gairdner, M. D. London, 1849. Post 8vo, pp. 239.

The Three Kinds of Cod-Liver Oil Comparatively considered, with reference to their Chemical and Therapeutic Properties. By L. J. de Jongh, M. D. of the Hague. Translated from the German; with an Appendix and Cases. By Edward Carey, M. D. London, 1849. 8vo, pp. 176.

A Dissertation upon Dislocations and Fractures of the Clavicle and Shoulder-Joint. Being the Jacksonian Prize Essay for 1846. By Thomas Callaway, F. R. C. S., Demonstrator of Anatomy to Guy's Hospital &c. London, 1849. 8vo, pp. 178.

Ueber Die Heilung der Jetzt Epidemisch Herrschenden Cholera gegründet auf die am Krankenbette gemachten Beobachtungen und Erfahrungen. Von Dr L. Grünberg Militär Arzte. Warschau, Leipzig, 1848. 8vo. Seite 36.

Memoire sur le Developpement, les Causes, et le Traitement du Cholera. Par Ch. Dvorjak, Medecin de la Cour Imperiale. Saint Petersburg, 1848. 8vo. Pp. 31.

The History of the Cholera in Exeter in 1832. By Thomas Shapter, M. D., Physician to the Devon and Exeter Hospital; the St Thomas's Hospital, near Exeter, for Lunatics; the Lying-In Charity, &c. London, 1849. 8vo. Pp. 297. Illustrated by a Map and numerous Woodcuts.

A Treatise on Cholera. By Nathaniel Alcock, A. B. M. B, Ex-Scholar of Trinity College, Dublin; Physician to Kilkenny City Gaol; and Surgeon to the County Infirmary; formerly Senior Medical Assistant in Townsend Street Cholera Hospital, Dublin, in 1832. London, 1849. 8vo. Pp. 189.

An Inquiry into the Bearing of the Earliest Cases of Cholera which occurred in London during the present Epidemic, on the strict Theory of Contagion; from No. 7 of the British and Foreign Medico-Chirurgical Review. By Edmund A. Parkes, M. D., Assistant Physician to University College Hospital. London, 1849. 8vo. Pp. 28.



On the Mode of Communication of Cholera. By John Snow, M. D. London, 1849. 8vo. Pp. 31.

The Harveian Oration, delivered before the Royal College of Physicians. By John Carr Badeley, M. D. Cantab., Fellow of the College. 1849. London. 8vo. Pp. 20.

The Microscopic Anatomy of the Human Body in Health and Disease. Illustrated with Numerous Drawings in Colours. By Arthur Hill Hassall, M. B., Author of a History of the British Fresh Water Algae; F. L. S., M. R. C. Surg., England, &c. Part xv. completing the work. London, 1849. 8vo. Pp. 443-570. Nine Coloured Plates. lxi.-lxix inclusive.

Norsk Magazin for Laegevidenskaben Udgivet af Laegiforeningen i Christiania. Ander Raekke. Redigeret af A. W. Munster, Lund. Voss. W. Boeck. Faye i. Band 11. Heft. Christiania, 1847. 8vo. From Pp. 583-650.

A Remonstrance with the Lord Chief Baron, touching the Case Nottidge *versus* Ripley. By John Conolly, M. D, Fellow of the Royal College of Physicians, London; and Physician to the Middlesex Lunatic Asylum at Hanwell. London, 1849. 8vo, pp. 16.

(Lunacy). Copy of a Letter to the Lord Chancellor from the Commissioners in Lunacy, with reference to their Duties and Practice under the Act. 8 and 9 Vict., c. 100. (Lord Ashley). London, 1st August 1849. 8vo, pp. 12.

Die Diagnostik Verdächtiger Flecke in Criminalfällen. Ein Physiologisch-Chemischer Beitrag zur Gerichtlichen Medicin. Von Carl Schmidt, D. et M. und Philosophie, Privat Docent zu Dorpat. Mitau und Leipzig, 1848. 8vo, Seite 48.

Annotations Cliniques sur la Phthisie Pulmonaire, D'Apres les Cas qui ont été traités a la Clinique de l'Ecole de Medecine de Rotterdam, Pendant le Cours de 1846-1847; Par M. Le Docteur G. P. F. Groshans, Lecteur a l'Ecole de Medecine de Rotterdam, Membre Correspondant de la Société de Medecine de Gand. Gand, 1849. 8vo, pp. 57.

The Retrospect of Medicine: Being a Half-yearly Journal, containing a Retrospective View of every Discovery and Practical Improvement in the Medical Sciences. Edited by W. Braithwaite, Lecturer on Obstetric Medicine at the Leeds School of Medicine, &c. Vol. xix. January-June 1849. London, 1849. 12mo, pp. 465.

The Half-yearly Abstract of the Medical Sciences: Being a Practical and Analytical Digest of the Contents of the Principal British and Continental Medical Works published in the preceding six months. Edited by W. H. Ranking, M. D. Cantab., late Physician to the Suffolk General Hospital. Vol. ix. January-June 1849. London, 1849. Post 8vo, pp. 400.

The London Medical Gazette, or Journal of Practical Medicine. No. 1124. New series 216. June 15 to 1137. N. S. 229. September 14, 1849.

The British and Foreign Medico-Chirurgical Review, or Quarterly Journal of Practical Medicine and Surgery. No. 7. July 1849.

London Journal of Medicine, A monthly Record of the Medical Sciences. No. 7, July; 8, August; 9, September 1849.

The Dublin Quarterly Journal of Medical Science. No. 15, August 1849. Dublin.

Weekly Tables of Mortality for the Metropolis. No. 24, 16th June 1849 to No. 37, 15th September 1849. Annual Series x.

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1ST JULY 1849.

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PART I.

ORIGINAL COMMUNICATIONS.

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ART. I.—*An Account of the Varieties of the Muscular System met with in the Dissecting Room of the University of Edinburgh during the Winter Session 1847-48.* By C. H. HALL-LETT, late Demonstrator of Anatomy in the University of Edinburgh.

THE following account of the various irregularities of the muscular system of the human body, noticed in the dissecting-room of the University of Edinburgh during the winter session 1847-48, is a continuation of the report,—published in this Journal (No. 174, Vol. lxix.),—of the varieties observed during the two preceding years.

It is necessary for me to mention, that the number of subjects dissected during the whole period over which my observations have extended, amounts to 200,—105 during the years 1846-47, and 95 during the winter of 1847-48. I mention this because it is requisite, that the absolute and relative frequency of any given variety should be determined. Indeed, this may be said to constitute the principal value of such observations, inasmuch as it enables the anthropotomist and the surgeon to form at once a pretty correct judgment of the nature and attachments of any variety which they may meet with during the dissection of the dead, or in operations on the living body, without exposing it completely.

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Before commencing the descriptions, I would remark that I have not deemed it incumbent on me to enter so much into detail in this as in my former report. It is only where there has occurred an important difference in the mode of attachment, in the absolute and relative frequency, or in the relations, that I shall have to advert to the varieties described in the first report, and then as briefly as possible.

*Musc. Retrahens Aurem.*—In two subjects, the lower fasciculi of this muscle were observed passing on and attached to the external occipital protuberance, and enclosed in a fibrous sheath, as before described. Altogether this variety has been noticed five times, or once in every forty subjects; and, in every instance, it was found in very robust male subjects. In one instance, this muscle was developed to a great extent, the whole of its fibres taking their origin from the occipital bone,—the lower from the protuberance, the upper from near the mesial line above it. It was not divided, as usual, into fasciculi, but formed a continuous flat muscle, nearly an inch and a-half broad at its origin.

*Musc. Risorius Santorini.*—This muscle, which is normally connected with the *Platysma myoides*, and usually arises from the skin and fascia over the masseter muscle and the parotid gland, was found, in the subject dissected for the lectures, to be attached firmly to the skin opposite the outer border of the superior third of the sterno-mastoid muscle. It consisted of a flat fleshy mass about two inches in breadth; and, passing directly forwards beneath the *concha auris* and over the parotid gland and masseter, became considerably contracted, and was lost in the angle of the mouth. A considerable space intervened between the *platysma myoides* and it.

*Musc. Digastricus.*—With the exception of the *biceps flexor brachii*, there is no muscle in the human body which so constantly presents a multiplication of its bellies. A third head was observed once in every fifteen subjects,—the proportion named in my first report. Two remarkable variations from the usual attachments of the additional head have fallen under my notice. The first of these was constituted by the origin of three pretty large fasciculi of fibres from the hyoid bone, near the mesial line, and on both sides of the neck, which, passing upwards, mingled with the innermost fibres of the normal anterior belly of the muscle near the lower jaw, and were inserted with it into the depression which the muscle normally occupies. This variety showed that the *digastricus* has a tendency towards the doubling of its anterior belly.\*

The second variety was still more remarkable. The third belly arose by muscular fibres from a tendon continuous with, and evidently an offset of, the intermediary tendon of the *digastricus*,

\* This variety is described by Soemmerring. De Corp. Hum. Fabrica.

and which passed inwards towards the mesial line, and over the smaller cornu of the *os hyoides*. The muscle passed forwards and upwards, and, having divided into two unequal portions, was inserted into the opposite side of the *symphysis menti*, previously interlacing with a corresponding abnormal head on the opposite side. Platner appears to have noticed a tendency to this variety, for he figures a third belly to the *digastricus*, which was inserted in the manner above described, but replaced the muscle of the other side.\*

In both these varieties, it is evident that the outermost fibres of the third belly, which have always a tendency to reach the symphysis of the lower jaw, were the only ones developed.†

*Musc. Stylo-Hyoideus*.—This muscle was found wanting, on one occasion, on the left side of the neck of a female subject, remarkable for the number of varieties by deficiency which it presented, not only in the muscular, but in the arterial system also. In some few instances, the *digastricus*, instead of perforating, passed wholly in front of it.

*Musc. Stylo-Glossus*.—This muscle presented a very uncommon variety in a subject in which an additional muscle, to be immediately described, was supplied to the pharynx. The variety consisted in its receiving additional fibres from the angle of the lower jaw, and also from the stylo-maxillary ligament. Some of these fibres mingled with those coming from the styloid process, but by far the greater number passed on separately to the tongue.‡

*Musc. Omo-Hyoideus*.—In my last report, I detailed the most important varieties of this muscle, tracing them through the different stages through which they appeared to pass; it is, therefore, unnecessary for me to repeat the descriptions, although I consider it incumbent on me to state, that every variety of this muscle, whether simple or complicated, is of the greatest importance, and that the descriptions I have already given have been fully confirmed by more extended observation.

The variety which, if met with in cases where it is necessary to perform operations in the subclavian triangle of the neck, would be most likely to confuse the operator, is where the omo-hyoid muscle has no attachment to the scapula, but takes its origin from the clavicle, in, but posterior to the interval between the sternomastoid and trapezius. I formerly stated that this variety was

\* De Musculo digastrico maxillæ inferioris. Leipsik, 1737. Tab. m.

† A cast of the second variety is placed in the anatomical museum of the University.

‡ Mozer, Deutsches Archives, &c., T. vii., p. 226, has apparently seen this variety; he describes the additional fibres as coming from the internal surface of the *pterygoideus internus*. In my own observation, they were traced with care to the stylo-maxillary ligament, as above-described. There can be little doubt that they are one and the same variety.

an uncommon one. I had then only seen it once; but I have now to state, that, in 100 subjects taken consecutively, it was found in no less than five, or, in the whole, in the proportion of three per cent.

On the right side of an adult male subject, the following variety was remarked. The posterior belly arose in its normal manner, but almost immediately divided into two, each being rounded and nearly of the same size as the ordinary posterior belly usually is. The superior division passed onwards, and gave rise, by the intermediate tendon, to the anterior belly, which was inserted into the hyoid bone normally. The inferior division passed into and mingled with the fibres of the sterno-hyoid muscle midway between the sternum and the hyoid bone, although the fasciculi of each were readily separated by the scalpel; it was inserted with the sterno-hyoid into the hyoid bone. Both posterior bellies were bound tightly down to the clavicle by the cervical fascia, and completely overlapped the subclavian artery in the third part of its course.\* Sels has noticed a special head pass into the sterno-thyroid muscle.† This variety is probably merely a modification of that in which the posterior belly ascends and passes into the sterno-hyoid or thyroid muscle.

The following variety presents the transition stage to complete absence of the muscle; it was observed on the left side of the neck of an adult male subject, the right omo-hyoid being attached to the clavicle as well as to the scapula. The posterior belly consisted of about a dozen muscular fibres, which gave rise to an exceedingly small tendon. The anterior belly arose from this tendon, and was nearly as large as it ordinarily is.‡

*Musc. Cephalo-Pharyngeus*.—The muscles of the pharynx are but little liable to variation, and when the varieties are noticed, they usually consist of nothing more than a more or less intimate connection with the muscles attached to the parts from whence the *constrictores pharyngis* take their origin. It occasionally happens, however, that the pharynx is furnished with an additional muscle, arising from a doubling of the stylo-pharyngeus, and the displacement of one of the divisions to either the temporal or occipital bones. Albinus and others have named this additional muscle, when it exists, the cephalo-pharyngeus. The only instance of this variety which has fallen under my notice, had the following attachments. It arose by an aponeurotic tendon, from the whole length of the vaginal process of the temporal bone, and, descending the neck with the stylo-pharyngeus, from which it was

\* A cast of this variety is placed in the museum of the University.

† De Muscul. varietat, Berlin, p. 6.

‡ This variety appears to be an extremely rare one. None of the older anatomists appear to have noticed it.

separated by the glosso-pharyngeal nerve, was lost amongst the fibres of the inferior constrictor muscle of the pharynx, spreading itself out on, and interlacing with, the fibres composing the outer surface of that muscle. This muscle was larger than the stylo-pharyngeus, and was present on both sides of the neck.\*

*Musc. Sterno-cleido-Mastoideus.*—I have already placed on record some instances of a double clavicular head. I have now to describe an instance in which the sterno-cleido-mastoid had four heads, two clavicular and two sternal. The second clavicular head arose from the clavicle in its ordinary position between the sternal and clavicular heads. The second abnormal sternal head was attached to the sternum, behind the normal head, and remained distinct from it to within an inch of the mastoid process of the temporal bone. All four heads were inserted together. I may also state here, that I have noticed the sterno-mastoid divided into two parts at its superior attachments, the muscle being bifid both above and below, but single in the centre.

*Musc. Trapezius.*—A tendency to deficiency of this muscle was noticed in three subjects, but in none to so great an extent as in the instance recorded in my last report. The trapezius in one subject was somewhat deficient on both sides,—the left more than the right. On the left side the muscle extended between the fourth cervical and the seventh dorsal spine; that on the right side was deficient in a corresponding degree below, but extended upwards to the occipital bone.

It is not uncommon to find the trapezius occupying the outer two-thirds of the clavicle when its occipital attachment is more extensive and more robust than ordinary. This has been remarked by all anatomists; but I am not aware that it has been seen by others so largely developed as not only to obliterate the space usually existing between it and the *sterno-cleido mastoid*, but also to overlap the clavicular head of the latter. I observed this in a male adult subject, remarkable for the large size of all the muscles on the neck, and the multiplication of others in the extremities.

This muscle in two instances was found to present a marked tendency to division into three muscles. The points at which the divisions existed were opposite the seventh cervical and the fourth dorsal spine. In both instances the lower division was more complete than the upper. This variety may afford a clue to the nature of the deficiency of parts of this muscle, either above or below, or in the middle, as observed by Soemmering.

*Musc. Latissimus Dorsi.*—The attachment of this muscle to the inferior angle of the scapula was not seen more than once in

\* More imperfect forms of this muscle have been described by Albinus, Sandifort, and others.



every six subjects, and then for the most part small and indistinct,\* showing, as I have stated elsewhere, that it ought to be considered an abnormal attachment.

The varieties presented by variations in the number of its attachments to dorsal spinous processes or to the lower ribs, were of frequent occurrence. These, however, were of no importance; but I have to record an instance in which the attachment to the ribs and lower part of the spinal column was normal, while the fibres attached to the crest of the ilium were wanting;—a remarkable circumstance, in as much as this is the most constant and least variable attachment of the muscle.

*Musc. Rhomboideus Major.*—This muscle often presented a marked tendency towards deficiency by a diminution in the number of its attachments to the dorsal spinous processes. With one exception, the only variety offered by this muscle was diminished size, from absence of the fibres usually attached to the third and fourth dorsal spines. The exception to this was observed in one of the subjects in which the trapezius was divided into three, and consisted in a more extended vertebral attachment than ordinary. In this subject, it was attached to six instead of four dorsal spinous processes.

*Musc. Levator Anguli Scapulae.*—It is not often that this muscle is abnormal. The most frequent variety consists in an additional slip from the fifth cervical transverse process, or more rarely from the mastoid process. I have seen three instances of the former and one of the latter variety. This muscle also gives off occasionally slips which pass in various directions, as to the rhomboid muscles, the posterior superior serratus, the clavicle, and the first and second ribs. I have also seen a large slip pass off from it near its centre, and running upwards and backwards, lose itself in the trapezius close to the occipital protuberance; apparently a rare variety.

*Musc. Sternalis Brutorum.*—I have already traced this remarkable muscle through its various stages, from a few muscular fibres placed between the *pectoralis major* and the skin, to the large, fleshy, and robust muscle extending between the *sternocleido-mastoid* and the *rectus abdominis*, and shall therefore confine myself to the description of one of its forms which I am not aware has been described by other anatomists. The muscle arose by a strong tendon from the sternal tendon of the left *sternocleido-mastoid*, with which it appeared to be directly continuous. The tendon passed over the manubrial portion of the sternum, and on the right side gave rise to a thick fleshy belly, which was inserted into the fourth costal cartilage near its sternal articulation, and sent off a tendinous prolongation to connect itself with the

\* The proportion given in my last report was one to three.

*rectus abdominis.* This muscle is usually attached to the costal cartilages or the rectus, or both, on the same side from which it takes its origin, but in this instance, it crossed from one side to the other, although still preserving its distinctive characters.

Many other slight modifications of this muscle were remarked, but I do not consider them worthy of especial notice. I may state, however, that it was once seen double on the right side, and single on the left, in the same subject. I need scarcely add, that this is a rare variety.\*

*Musc. Pectoralis Major.*—The only variety of this muscle which I believe to be worthy of record, consisted in the complete separation of that portion which takes its origin from the abdominal aponeurosis from the sternal, and this from the clavicular portion. The sternal and clavicular portions were inserted normally into the interior of the bicipital ridge. The lower portion continued distinct from the rest throughout, crossed the axilla behind, and somewhat beneath the lower portion of sternal part of the muscle, and was inserted into the common tendon of the short head of the biceps and *coraco-brachialis*, about an inch below the coracoid process. The *pectoralis major* not unfrequently presents a tendency to division, and occasionally sends detached fibres to the biceps and *coraco-brachialis*; in the variety above described, we have a combination of two of its most common anomalies, and that more fully marked than usual.

*Musc. Pectoralis Minor.*—This muscle often varies in the number of its attachments to the ribs. In one subject, in which it was very large and robust, it arose from five ribs, from the second to the sixth inclusive; the superior portion was inserted as usual into the coracoid process, but the lower part passed towards the humerus, and was inserted by means of tendinous fibres into the anterior bicipital ridge, behind the tendon of the *pectoralis major*.

*Slips of Muscle in the Axilla.*—In my former report I described some remarkable slips of muscle passing through the axilla in connection with some varieties of the *pectoralis major*. In this one, however, I have thought it better to place them by themselves. The minor varieties I shall not notice, but the two following anomalies are, I believe, worthy of record.

A slip of muscle about an inch in breadth was observed crossing the outer part of the axilla, and passing obliquely over the axillary vessels, and the nerves in relation with them, at the point where a ligature is placed, when necessary, on the axillary artery in the lower part of its course. This slip of muscle was connected to the *latissimus dorsi*,—its muscular portion,—by inter-

\* A cast of this variety, and of one single on each side, is preserved in the museum of the University.

mediate tendinous fibres situated about two inches distant from the shaft of the humerus. In ordinary cases, this slip would have passed forwards to and have been blended with the *pectoralis major*, or have been connected with the coracoid process of the scapula; but in this instance, it was inserted by an aponeurosis into the fascia of the arm immediately over the *coraco-brachialis* and biceps, about two inches below the coracoid process. It was found in both axillæ, and is of importance to the surgeon.\*

The following abnormal muscle might perhaps have been best described amongst those of the muscles of the arm, but as it had no connection with those muscles, and passed through the axilla, I have noticed it in this place. It arose by an aponeurotic tendon from the whole length of the inner border of the coracoid process of the scapula. This tendon immediately gave rise to a pretty large muscular belly, about three inches in length, and three-quarters of an inch in thickness, which passed downwards over the tendon of the *subscapularis* muscle, and the inner part of the capsular ligament of the shoulder joint, and was inserted immediately above, and nearly in a line with the lower border of the tendon of the *latissimus dorsi*, into the shaft of the humerus. Before it was inserted into the humerus, it received the fibres of a second and much smaller belly, coming from the capsular ligament of the shoulder joint, to which it was attached by a thin tendon, from whence the muscular fibres originated.†

*Musc. Serratus Magnus.*—I have elsewhere described an instance in which the middle fibres of this muscle were wanting, an abnormal condition of the muscle which has been noticed and described by other anatomists. I have here to record a variety which appears to be a transition stage towards the formation of that rare anomaly. The variety occurred in an adult female subject, which presented others by redundancy. The muscle had its usual number of slips from the ribs, and all were apparently of their normal size, but in passing towards the posterior or vertebral border of the scapula, they became collected into two large bundles, separated from each other by a cellular interval. The superior bundle was inserted into the upper third, and the lower bundle into the lower third of the vertebral border of the scapula, the middle third being unoccupied. There was thus a triangular interval left between the separated portions of the muscle, the

\* A cast of this abnormal slip is preserved in the museum of the University. No less than thirteen other varieties in the muscular system were discovered in the subject which presented this important anomaly.

† A cast of this remarkable abnormal muscle is placed in the museum of the University. Its superior attachments would lead one to suppose it merely a portion of the biceps, which, in other subjects, might have resulted in a multiplication of its heads.

apex being at the sixth rib, and the base at the vertebral border of the scapula.

Another remarkable variety of this muscle consisted in the greater part of the lower fasciculi, those connected with the 7th, 8th, and ninth ribs being wholly unconnected with the scapula. They were displaced, and were inserted by tendinous fibres into the vertebral aponeurosis, which they strengthened considerably.

*Musc. Teres Major.*—In the right shoulder of the subject in which one of the abnormal slips in the axilla before described was found, the following variety of the *teres major*, a muscle rarely abnormal, was observed. It was triple, the three portions being for the most part distinct throughout. The lower division of the muscle presented the normal attachment to the scapula. The middle division, more than an inch in breadth, arose in a line with the lower from the axillary border of the scapula, and extended as far outwards as the glenoid cavity. The superior division, which appeared to be an offset of a very robust *subscapularis*, also arose from the same border of the scapula, close to the glenoid cavity. They all passed outwards towards the humerus, the middle head folding behind, and becoming partially connected with the inferior one; both these were inserted together in the normal place. The superior head was also inserted into the posterior bicipital ridge of the humerus, in a line with the others, but its tendon was only slightly connected with that of the other two heads, and that only at the point of insertion.\*

*Musc. Teres Minor.*—This muscle is often so intimately blended with the *infra spinatus* muscle, that it is impossible to define it, or show it according to the descriptions given of its attachments, without very free use of the scalpel. In nearly one half of the subjects I have dissected, or have been dissected under my superintendence, no cellular interval between the two muscles could be detected. I direct attention to this, because it is necessary that the normal condition of the muscle should be defined.

*Musc Biceps Flexor Brachii.*—Thirteen subjects presented a third head to the biceps, that is, about one in every eight subjects; the same proportion was observed by Thiele. In the previous session, the third head was found but once in every fifteen subjects.

I have elsewhere stated that Meckel and others had seen the third head of the biceps consisting of a detached portion of the *brachialis anticus*, and therefore, when highly developed, and the connection not apparent, it may be considered as an offset of that muscle. The following curious and interesting variety supports that inference. The biceps had three heads, two of which were normally attached. The third head was confounded with the

\* A cast of this variety may be seen in the museum of the University.

upper part of the inner portion of the *brachialis anticus*, became detached from it as it descended the arm, and was inserted by two tendons into the upper and lower borders of the tubercle of the radius, distinct from the proper tendon of the biceps, which was placed between the two tendons of the additional head. The third head may therefore have three different destinations; it may be blended with the belly formed by the two normal heads, and this is by far the most common mode of termination; or it may be distinct from the biceps muscle, but give attachment to its semilunar process to the fascia of the fore-arm; or, lastly, be distinct throughout, and inserted separately into the tubercle of the radius.\*

The subject dissected for the lectures had the third head arising in its usual place in the left arm; but in the right, it arose from the capsular ligaments of the shoulder-joint between the two tuberosities. This variety is interesting, inasmuch as it shows that the *fourth* head of a quincipital flexor, such as I described in my first report, may exist without the *third* head being present.†

*Musc. Triceps Extensor Brachii.*—This muscle is but little liable to variation; the two following anomalies are therefore worthy to be recorded. The one consisted in the external head being double, the two portions being evidently produced by the division of the normal external head throughout its whole length. The other consisted of a fourth head arising from the surgical neck of the humerus between the lowest points of insertion of the *sub-scapularis* and *teres minor* muscles. This head passed down the arm and was inserted into the anterior surface of the tendon common to the long and the external head, near the commencement of the lower third of the arm, having previously sent off a considerable bundle of fibres to interlace and be confounded with those of the internal head.

*Musc. Flexor Carpi Radialis.*—This muscle is also one of those little subject to variation. The only variety this muscle has presented to my notice is the following. It arose by two heads, the one normally, the other, by means of a thin aponeurosis, from the oblique ridge of the radius, parallel to, and somewhat above, the radial head of the *flexor sublimis digitorum manus*. The muscular fibres attached to this aponeurosis converged to a point below and were connected with those of the normal head about the com-

\* Casts showing these modifications and many others may be seen in the museum of the University. The second modification was fully described in my former report. The two last, so far as I am aware, have not been noticed by other anatomists.

† A cast of this variety is placed in the museum of the University.

mencement of the lower third of the fore-arm. The tendon was inserted normally.\*

*Musc. Pronator Radii Teres.*—The following variety of this muscle is of so much importance in a surgical point of view, as to render it unnecessary for me to offer an excuse for describing it in detail.

The muscle arose normally from the internal condyle, &c., and near its insertion into the radius received the fibres of a considerable muscular belly, which had the following abnormal attachment above. It arose by two heads: one from the internal condyloid ridge between the internal head of the *triceps extensor* and the *brachialis anticus*; the other, from the internal intermuscular septum,—that portion which represents, and is occasionally converted into the *humeral foramen*. The two heads were connected together by an aponeurosis, from which fibres also took their origin. The attachments of this abnormal head of the round pronator of the fore-arm, however, would be of little interest, were it not for the alterations it produced in the relations of the brachial and radial arteries. The brachial artery, together with its *venæ comites* and the median nerve, were drawn inwards, constricted and covered by the muscle for about an inch and a half above the bend of the elbow. The radial artery passed out from the bend of the elbow between the normal and abnormal bellies, and was subcutaneous throughout the whole of its course down the fore-arm.†

This variety appears to be a highly developed form of that figured by Professor Quain in his exquisite plates showing the surgical anatomy of the arteries.

*Musc. Palmaris Longus.*—In my first report, I stated that this muscle was found deficient in both arms once in every three subjects, and in a considerable moiety of the remainder it was deficient in one or other of the extremities. I have now to report its deficiency once in every seven subjects in the last hundred dissected, or once in every five subjects in the totality.

When deficient it is most commonly replaced by a slip sent off by the *flexor sublimis digitorum*. The following variety shows that this slip may be present when the muscle is normal. The *palmaris longus proper* arose from its usual points of attachment, and its tendon passed into the *palmar fascia*, having but very

\* A cast of this variety is preserved in the museum of the College.

† The extremity and a cast of the parts are preserved in the museum of the University. It may not be uninteresting to remark, that the superficial veins were abnormal; the cephalic vein and the veins communicating with the deep veins at the bend of the elbow being wanting. (This anomaly of the superficial veins is fully described in my paper on the anomalies of the venous system.—Medical Times, Nov. 13, 1847.)



slight attachments to the annular ligament. The abnormal muscle, placed internal to the normal one, was a highly-developed form of the offset from the *flexor sublimis digitorum*; its tendon passed into the annular ligament of the carpus.

In one instance, where the muscle was more highly developed than usual, it was completely divided into two in the longitudinal direction. The outer of these divisions was inserted into the scaphoid bone and the outer part of the annular ligament. The inner portion had its tendon double, one of which was attached to the pisiform bone and the inner part of the annular ligament, while the other gave rise to a fleshy belly which passed over the annular ligament, the ulnar artery and nerve, and terminated in a small round tendon, which blended with the fibres of the *abductor minimi digiti*, near its insertion into the base of the first phalanx of the little finger.

In two subjects the muscle was found completely doubled, each portion being inserted separately into the annular ligament and palmar fascia.

*Musc. Flexor Sublimis Digitorum.*—This muscle is but little liable to variation, unless we consider its occasional connection with the *flexor longus pollicis* as a variety. I have to state, that in two subjects,—both female,—the tendon to the little finger was absent in both fore-arms; but in neither case was it replaced by the deep flexor as seen by Quain.

In one subject, in which the muscle was large and robust, the fibres attached to the oblique ridge were found wanting.

*Musc. Flexor Profundus Digitorum.*—This muscle was found abnormal in four subjects. In one it had an additional head arising, by means of fleshy indigitations, from the upper three-fourths of the inner border of the radius, and from the interosseal ligament; the greater number of the fibres converged to a point near the wrist-joint, and passed into the tendon destined for the index finger, while others, after passing some distance down the fore-arm, curved upwards and outerwards, and were lost in the *flexor longus pollicis*.\*

In another subject, the outermost fibres of the muscle,—that portion connected with the tendon sent to the index finger,—were completely separated from the rest. This separated portion arose by two heads, both tendinous. The internal head was attached to the base of the coronoid process, to about an inch of the outer part of the ulna, and to nearly one-half of the interosseous membrane. The internal head arose from the internal border of the radius, immediately below its tubercle, and from the interosseous membrane. The muscle passed down the fore-arm between the

\* A cast of this variety is preserved in the museum of the University.

other portion of the deep flexor of the fingers and the *flexor longus pollicis*, and, near the wrist, gave rise to a tendon which was sent to the index finger. The anterior interosseal artery and nerve passed between the two heads of this separated portion of the deep flexor.\*

Another subject also had the outermost fibres completely separated from the rest, and forming a proper flexor of the index finger. In this instance, however, the muscle was only attached to the ulna and the interosseous membrane, the radial head seen in the former variety being absent.

In all of these varieties, which are mere modifications of each other, we observe the marked tendency to the formation of a special flexor of the index finger, always presented by the deep flexor, fully carried out. The following variety, however, shows that this tendency to division and separation is not confined to the portion acting on the tendon of the index finger. Some of the anterior and internal fibres of the muscle had been detached, and rested in the cellular interval between the superficial and deep flexors of the fingers. These detached fibres arose from the internal condyle of the humerus and the coronoid process of the ulna. Below it passed into a tendon, which was inserted into the distal phalanx of the little finger, after sending off a slip, in the palm of the hand, to be connected with the tendon for the ring finger, and coursing through the loop formed for it by the corresponding tendon of the superficial flexor. A special flexor of the little finger, therefore, existed in this subject.†

*Musc. Extensor Carpi Radialis Longior.*—This muscle was once seen divided into two, in the direction, and throughout the whole, of its length. The two portions were of unequal size. The larger was inserted into the metacarpal bone of the index finger as usual. The smaller was also inserted into the same bone in advance of the larger, and also sent a slip to the fascia at the wrist.

*Musc. Extensor Minimi Digiti.*—This muscle was found wanting once in every fifteen subjects. In all these cases, with the exception of one, it was replaced by a slip from the *extensor carpi ulnaris*. The *extensor communis digitorum* may also replace it, as stated in my first report; but no instance of this occurred in the last hundred subjects dissected.

In one subject, the muscle was found wanting in the left arm, with no indication of an attempt at its replacement by other muscles. In the right arm, however, the muscle was found completely double, a cellular interval separating the two portions,—a rather remarkable circumstance,—and both portions passed on to the little

\* A cast of this variety is preserved in the museum of the University.

† A cast of this muscle may be seen in the museum of the University.

finger. This variety was associated with deficiency of the tendon from the *extensor communis* to the little finger.

*Musc. Extensor Carpi Ulnaris*.—This muscle was seen, in one instance, to send off a slip of its tendon to the innermost tendon of the extensor of the finger, the proper extensor of the little finger being also present. This variety is of physiological interest, inasmuch as it shows, that, where a muscle is present, the muscles in its vicinity, which usually replace it when absent, may still develop the accessory portion. I have already noticed another instance of this in connection with the *palmaris longus*.

*Musc. Extensor Ossis Metacarpi Pollicis*.—I have elsewhere stated that this muscle generally presents a tendency to divide into parts near its insertion, and that this arrangement, from the frequency of its occurrence, ought to be considered the normal condition of the muscle; and also that this tendency to division not unfrequently resulted in the doubling of the muscle. I have now to record the most extensive and most extraordinary subdivision of this muscle which has probably ever been witnessed by the anatomist.

The muscle was divided primarily and completely into three,—an internal, a middle, and an external extensor of the metacarpal bone of the thumb; a distinct cellular interval intervening between each of them. The external and middle portions were again subdivided through the whole of their tendons, and through fully one-half of their fleshy bellies; so that there was exhibited a tendency to the formation of no less than five *extensores metacarpi pollicis*, three of the five being completely formed. The superior and external division was inserted into the *palmar fascia*, and part of it was also continuous with the short flexor of the same bone. The other four tendons were attached, one after the other and separately, into the metacarpal bone of the bone, from its base to near its centre.\*

The muscle was seen completely double, and the tendons inserted, as described in my first report, in five subjects, or in the proportion of one to twenty.

*Musc. Extensor Indicis*.—I would here again call the attention of anatomists to the frequent occurrence of more or less complete division of this muscle, one of the divisions forming a more or less perfectly developed proper extensor of the middle finger,—a muscle which exists normally among most of the quadrumana. It was found completely divided into two in ten subjects, and partially divided in twelve others.

*Musc. Abductor Pollicis*.—In one subject, in which the *extensor ossis metacarpi pollicis* was found double, this muscle was

\* A cast of this most extraordinary instance of multiplication of muscle is preserved in the museum of the University.

divided into two in the direction of its length; a cellular interval existing between the two portions. Both of them were attached to the base of the first phalanx of the thumb; but the more external of the two was considerably strengthened by fibres connected with the tendon of the *extensor ossis metacarpi pollicis*.

*Musc. Palmaris Brevis*.—The most common variety of this muscle is its complete deficiency, a tendency to this being frequently observed in the paucity and the extensive separation of its different fasciculi. Such being the case, it may not be uninteresting to record an instance of its enlargement. The muscle arose from the annular ligament of the *carpus* and the *palmar fascia* throughout the whole extent of the depth of the palm of the hand. The fasciculi were numerous and closely packed together, the whole forming a thick fan-shaped muscle, which was inserted into the skin as usual, but over a greater extent than ordinary.\*

It may be remarked, that oftentimes, when this muscle is supposed to be absent, careful dissection will display one or more small pale fasciculi of fibres.†

*Musc. Flexor Brevis Minimi Digiti*.—I have elsewhere recorded instances of the occurrence of a second head to this muscle, and I have in this report described a modification of this additional head in connection with the *palmaris longus* muscle, the additional head being displaced and connected with the abductor of the little finger. That such is the proper interpretation of the anomaly, is sufficiently shown by the instances I have described of numerous varieties of the *palmaris longus*, especially where it is replaced by other muscles, of the short flexor of the little finger, and also by the following anomaly.

A muscular slip arose from the deep surface of the *anti-brachial aponeurosis*, about two inches above the wrist-joint, passed downwards over the annular ligament, and concealing the ulnar artery and nerve, and, in the palm of the hand, gave rise to a small thin tendon which passed partly into, and intermingled with, the fibres of the *flexor brevis minimi digiti*, while another part was inserted into the inner side of the base of the proximal phalanx of the little finger.

This muscle is often connected with the annular ligament and palmar fascia, especially when the *palmaris longus* is absent.

*Musc. Triangularis Sterni*.—The abnormal conditions of this muscle, as described in my last report, were all found occasionally during the last year. The muscle, however, was not found completely deficient, as previously reported, in consequence, probably,

\* A cast in the museum of the University.

† I stated in my first report, that it was principally from careless dissection that this muscle was reported as deficient.

of a better mode of displaying it having been adopted, although the upper or the lower fasciculi were occasionally found wanting.

The usual tendinous, or rather aponeurotic connection between this muscle and the *transversus abdominis*, was found converted into muscle in four subjects. The *triangularis sterni* and *transversus abdominis* were therefore only continuous and inseparable once in every twenty-five subjects. The small proportion of subjects in which this condition of the two muscles is found, strengthens my opinion, stated in the first report, that Rosenmüller's proposition to describe them as one under the name of sterno-abdominalis, cannot be adopted, although the one is evidently a repetition of the other.

*Musc. Obliquus Externus Abdominis*.—A very robust male subject presented a remarkable deficiency of two of the digitations of this muscle. The fasciculi attached to the eighth and ninth ribs were wanting. A large hiatus in the muscle was thus produced between the digitations attached to the seventh and tenth ribs respectively. It was about two inches and a-half in extent, and was filled up by a very strong fibrous tissue. The tendon was complete.

*Musc. Pyramidalis*.—The first hundred subjects dissected presented this muscle deficient in the proportion of one to three; in the second hundred, however, it was found deficient once in every fifteen subjects; in the totality, therefore, it was wanting in the proportion of one to nine or ten.

*Musc. Psoas Parvus*.—I have already drawn the attention of the profession to the contradictory statements made by different anatomists with regard to anomalies of this muscle. I then stated that the *psaos parvus* was more frequently present than absent, the proportions being 61 to 54, and this proportion was preserved throughout the second hundred subjects. The remark made by Thiele,\* that he had opened twenty subjects consecutively, without having met with it once, is no doubt correct. I have myself seen it wanting in seventeen subjects, opened one after the other, and without regard to sex, but in the succeeding seven it was present in all. It is therefore necessary that a very large number of subjects should be dissected, before a conclusion should be drawn concerning the normal condition of a muscle so variable as this one evidently is. It is also necessary for me to remark, that when present, it will be most frequently found but on one side of the body, and that usually the right.

The assertion of Riolan,† that he had never seen the muscle in females, is strangely at variance with the statements of Wins-

\* Encyclopedie Anatomie, Vol. iii. art. *m. petit psaos*.

† Anthropographia, Vol. 5, p. 508. -

low\* and John Bell,† and others, that it is more frequently met with in females than in males. In the first hundred subjects dissected, it was found as often deficient in the female as in the male; but, in the second hundred, it was more frequently present, in the proportion of seven to six. More extended observation may reveal some curious statistics of this muscle.

*Musc. Psoas Magnus*.—This muscle is but little subject to variation. The following is the only one which has fallen under my notice. The fibres attached to the first and second lumbar vertebræ were separated from the rest of the muscle, and passed onwards towards the thigh in a sulcus between it and the *iliacus internus*, the anterior crural nerve being placed first between the detached portion and the *iliacus internus*, and subsequently between the two portions of the muscle. Immediately below Poupart's ligament, the muscular fibres, collected into a belly about half an inch in breadth, gave rise to a tendon which immediately split into two;—one short and expanded, which intermingled with some of the fibres of the *iliacus internus*; the other, long and thin, like the tendon of the plantaris, which continued onwards in the direction of the combined flexors of the thigh, and was inserted distinct from both into the lesser trochanter.‡

This variety is a modification of one seen occasionally by Albinus§ and Meckel,|| the crural nerve having a slightly different relation to it. Meckel also states that it establishes an analogy with the great psoas in the apes.

*Musc. Pyramidalis*.—This muscle was occasionally found divided into two unequal portions, the peroneal division of the great sciatic nerve passing between them. In one subject, the muscle was divided into two, but the lesser sciatic, and not a division of the greater sciatic nerve, passed out between them.

*Musc. Gemellus Superior*.—This muscle was found wanting on both sides in five subjects, and on the left side only in seven others. It was once observed double in the right hip of a male subject, the greater part of the great sciatic nerve passing between the two portions.

*Muscular slips in the Popliteal Space*.—In the middle of the popliteal space of an adult female subject, there existed the following abnormal muscle. It arose from the middle of the lower part of the femur, immediately above the condyles, and, passing superficially to the popliteal vessels and nerve, terminated in a small tendon which attached itself to the tendon of the gastrocnemius, where the two heads meet. It was closely applied to

\* Exposition anat. struct. corp. hum., Vol. ii., p. 211.

† Anatomy of the Bones, p. 341.

‡ A cast of this variety may be seen in the museum of the University.

§ Historia Musculorum Hominis, p. 315.

|| Anatomie Descriptive, C. Grand Psoas.

the plantaris as it descended; and this circumstance, combined with its attachments, has led me to regard it as an additional plantaris muscle; but, as this conclusion may not be generally received, I have described it in this place.\*

The following abnormal muscle is probably a modification of the preceding. It arose from the depression above the external condyle of the femur, and above the origin of the plantaris, crossed the floor of the popliteal space beneath the popliteal vessels, and was inserted by tendinous fibres into the *ligamentum posticum Winslowii*. The muscle was as large as the plantaris.

*Musc. Adductor Longus*.—This muscle was once observed divided into two throughout its whole length. The two divisions were placed side by side, the superior being the smaller. The lower perforating artery sent off, in this subject, from the great muscular branch of the superficial femoral, passed to the back of the thigh, between the two portions near their insertions into the femur.

*Musc. Adductor Brevis*.—The following supernumerary adductor of the thigh may be considered a variety of the short adductor, and, as such, I shall describe it. It arose from the *os pubis*, immediate above and in front of the origin of the *adductor brevis*, and, passing outwards, downwards, and backwards, first in front of the *adductor brevis*, and then behind the pectineus, was inserted into the posterior part of the small trochanter, behind the psoas and iliacus.†

*Musc. Gastrocnemius*.—This muscle received a slip of muscular fibres, about one inch in breadth, from the head of the fibula above the origin of the soleus. This slip of muscle was inserted into the tendinous part of the outer head of the gastrocnemius, near its union with the soleus, to form the *tendo Achillis*.

*Musc. Soleus*.—The tendency to the doubling of this muscle, by splitting in the direction of its length, was noticed in three subjects, but not to the same degree as in those described in my first report.

*Musc. Plantaris*.—That Meckel must have fallen into error when he stated that the plantaris was more inconstant than the *palmaris longus*, is evident from the observations of Gautzer, and from the fact, that while the *palmaris longus* was often wanting, the plantaris was only absent once in the whole 200 subjects over which my observations extend.

The following varieties of this muscle are somewhat remarkable. In a male subject, in which several anomalies by deficiency were detected, the plantaris was found to have two heads, separated

\* A cast of this variety is placed in the museum of the University.

† A cast of this supernumerary adductor is preserved in the museum of the University.

from each other by a distinct cellular interval. One of the heads arose from the space immediately above the external condyle of the femur. The other was attached also to the femur, below the first head, to the posterior ligament of the knee-joint, and to the tendon of the gastrocnemius; it was larger than the superior head, and, at the same time, broader and thicker. The superior head gave rise to the tendon into which the fibres of the inferior head were received. The tendon was nearly twice its ordinary size.\* The muscle was here evidently more robust than usual.

The same variety was found in the left lower extremity of an adult female subject. In the right lower extremity, however, the attachments to the femur were wanting, and that part of the muscle only which corresponded with the inferior head of the fellow in the opposite extremity was developed. These varieties indicate that the plantaris, when fully developed, is a double-headed muscle; that, in a large proportion of subjects, the superior head is wanting; and that, although they are sometimes seen together, they may either of them exist independent of the other. The slips seen in the popliteal space, and already described, appear to me to be nothing more than varieties of this superior head of the plantaris.†

*Musc. Extensor Hallucis.*—In four subjects the extensor muscle of the great toe was found divided through nearly its whole length into two unequal portions, the internal being invariably much larger than the external. The internal division was inserted in the ordinary manner. The external gave origin to a long thin tendon, which passed through a distant sheath in the annular ligaments of the ankle, and was inserted into the base of the proximal phalanx of the great toe, external to, and distinct from the tendon of the internal extensor.

This variety, or modifications of it, is the only one to which this muscle appears to be liable. Thiele‡ and Meckel|| have both seen it completely double. It is an interesting and important variety, inasmuch as it approximates the muscles of the great toe to those of the thumb.

*Musc. Extensor Brevis Digitorum Pedis.*—In seven subjects this muscle was observed splitting into five instead of four portions, the additional portion being placed between that passing to the great toe and that for the second toe. The tendon of this additional slip did not pass to either of the toes, but was invariably attached to the metatarsal bone of the second toe, and to

\* A cast of this variety is preserved in the museum of the University.

† Thiele was aware of the occasional existence of a second head arising from the femur. *Op. cit.*, p. 317; and *Hallische Literaturzeitung*, 1808, No. 153. Casts of the modifications of this muscle are preserved in the museum of the University.

‡ *Op. cit.*, p. 308.

|| *Op. cit.*, extensor hallucis longus.



the tendon of the first dorsal interosseous muscle. In one subject all the tendons were found double, one being inserted normally, the other being attached to the metacarpal bones and to the tendons of the interosseous muscles.\* The first variety was described by Albinus,† and has been seen by Meckel,‡ Thiele,§ and others. Meckel considers it to be an attempt to form an indicator muscle corresponding to, and repeating that for the index finger of the hand.

The internal slip was often seen completely separated from the rest, and in one subject the four slips were separated from each other.

Besides the varieties which have been described in this report, many others were met with, which, for the reason set forth at the commencement, have not been noticed here. The most common of these occurred in about the same proportion as described in the first report; others, of minor importance, were not met with so often.

The extent of the general remarks appended to the last report, renders it unnecessary for me to say more than, that more extended observation has tended to strengthen my opinion concerning the correctness and general application of the rules therein advanced.

ART. II.—*On Plague, in relation to the Question of its Nature, whether or not a Contagious Disease.* By JOHN DAVY, M.D., F.R.S., Lon. and Edin., Inspector-General of Army Hospitals, &c.

THERE are two methods of investigation applicable to this subject,—one, which consists in taking as general a view as possible of the disease, including all that is known, or is supposed to be known of its spread and propagation; the other, in narrowing the sphere of vision, and fixing the attention on a small number of particulars.

Of the two methods, each has, or appears to have, its advantages. That embracing the greatest generality, seems best fitted to carry conviction home to the mind, and to determine the truth in a comprehensive manner. The latter, limited in its bearing, is recommended by the facility of conducting it,—the precision

\* A cast of this variety is placed in the museum of the University.

† *Op. cit.*, p. 602.

‡ *Op. cit.*, court extensor commun des orteils.

§ *Op. cit.*, p. 323.

which it admits,—and its freedom from various sources of fallacy to which the other is exposed.

On the present occasion, in the observations which I shall have the honour to submit to the profession, I propose to follow the second method, not only as best adapted to the narrow limits of a paper, but also, as I believe, most suitable to the subject of inquiry, which, in all its generalities, has become so complicated, confused, and difficult.

Always in physical research, *experimenta*, or *instantiæ crucis*, have been considered desirable, and have been anxiously sought for in proportion to the obscurity of the matter under investigation. The cases I am about to make mention of, if I do not err, are facts of the kind, and therefore, I would hope, deserving of consideration, and of being thus brought forward.

In the year 1841, in the month of June, a vessel arrived at Constantinople from Alexandria, where at that time plague prevailed in a mild form, to no great extent. It was found that the disease existed amongst the crew and passengers, to some of whom it had proved fatal on the voyage. All the survivors were landed at the lazaretto on the Asiatic shore of the Bosphorus, or on the uninhabited island of Proti, in the Sea of Marmora adjoining, and were placed in strict quarantine, together with their effects, and everything considered susceptible belonging to the vessel.

Of the crew and passengers (altogether about 96), 6 came under treatment in the lazaretto, as cases of plague, and 3 in Proti,—of whom 7 died.

Had the disease limited itself to those who came from Egypt, the instances would hardly have been deserving of remark. It might be said,—the measures of quarantine had been successful in stopping its extension; or, by those who do not believe the disease to be capable of spreading by contagion, that the local causes required for its production, were, at that time, not existing in the lazaretto, or not in activity there, and therefore that its not spreading was no more than might have been reasonably expected,

But the disease was not thus limited. Amongst the persons belonging to Constantinople, employed in the service of the lazaretto, strictly confined within its walls, or allowed to communicate only with the infected ship, four contracted the malady. These were two young persons, the son and daughter of the purveyor of the establishment, and a guardiano and a porter, one a young man of 19, the other of middle age; three of whom died, the porter only recovering.\* In a return appended to a letter, specially on the subject of these cases, which M. Pezzoni, a distinguished member of the Superior Council of Health in Constanti-

\* The guardiano alone communicated with the ship.

nople, did me the honour of addressing to me, and which, it being his wish to make public, has been communicated to the Medico-Chirurgical Society of London,—the names of these individuals will be found, with a notice of some of the principal circumstances of their illness, drawn up and certified by the Inspector-General of Quarantine at Constantinople, and accompanied by a similar notice of all who were under treatment,\* not including, however, besides, amongst those already generally alluded to, a lay monk from Syria, in whom the disease showed itself, or was discovered, not when he was in the lazaretto, but subsequently in Pera, the day after his liberation from quarantine.

As regards the four cases in persons belonging to Constantinople, to whom particular attention is due, and also the last mentioned, three questions may, with propriety, be asked, and require to be answered. 1st, Is it certain that the disease under which they laboured was genuine plague? 2d, Is it certain that Constantinople, with its suburbs, at the time was free from plague? And, lastly, is it clear that the monk, in whom the disease appeared, contracted it in the lazaretto and not in the city?

If satisfactory answers can be made to these questions, I apprehend, the instances, the four cases specially brought forward, may deserve to be considered as *instantiæ crucis*,—proofs demonstrative that the plague admits of being propagated by contagion; whilst, on the contrary, if the answers should be unsatisfactory, the cases adduced, in the light of evidence, can be held to be of no value.

I need hardly remark, that, as far as my own conviction is concerned, I am satisfied that each question admits of an answer in the affirmative, which was also the firm persuasion of all the members of the Superior Council of Health, most of them men experienced in the plague,—holding very responsible situations, and having no motive to conceal the truth, or to state otherwise than their sincere belief,—the majority of them, as I mentioned in my former paper, being delegates from the different embassies in Constantinople.† Twice I visited the lazaretto with some of these gentlemen, during the period under consideration, and saw the patients; and I often had opportunities of conversing with one or other of them on the subject; and neither at the time of our visits, with the patients before us, nor afterwards on reflection, did there seem to be the least ground for suspicion that the malady was not the plague.

It is not necessary to dwell on the symptoms present. Accom-

\* Notice of Cases of Plague contracted in the lazaretto of Constantinople, in a letter addressed to Dr Davy. By M. Antoine Pezzoni, Conseiller de l'Etat de S. M. l'Empereur de toutes les Russies, &c. With Remarks by Dr Davy. Transactions of Medico-Chirurgical Society, Vol. xxv. pp. 167 and 189. London, 1842.

† Transactions of Royal Society, Edinburgh, Vol. xv. part ii. p. 311. Edin. 1842.

panied with buboes or carbuncles, they were considered by the most competent judges familiar with the plague to be characteristic of it,—a conclusion that was corroborated by the high proportional mortality of the disease, and that at a time when no other disease likely to be confounded with it was known to prevail.

That the plague, then, did not exist in Constantinople, or its suburbs, or in the adjoining country, appears to have been a well-established fact. For three years the city had been exempt from it; and, just at the time of the importation of the disease into the lazaretto from Alexandria, Constantinople had the advantage of being in free communication with Vienna and the Austrian territories by way of the Danube,—a privilege not before conceded, I believe, for a century, and granted on account of absence even of suspicion of danger. Whilst plague was within the walls of the lazaretto, it is true that reports more than once spread in the city, that the disease had broken out amongst the inhabitants; but, on careful inquiry in each instance, they were found to be groundless alarms, arising out of some unusual case of sickness totally different from plague.

The instance of the monk was clearly established, as not an exception to the preceding remark. From his own statement, it appeared that he was taken unwell before he quitted the lazaretto; that he concealed his indisposition, anxious to be at liberty; and that he did not disclose it till he became alarmed for his life, on the discovery of a carbuncle over the right iliac region. He was residing in his convent when this symptom occurred, and no suspicion even of plague was attached either before or after to any other of its inmates. Of course, as soon as the disease was made known, he was replaced in quarantine, where I had an opportunity of seeing him.

Should the statements just made appear to be satisfactory in relation to the questions proposed, is not the conclusion I alluded to, that the cases under consideration are *instantiæ crucis*, unavoidable? Are they not, if I may say so, without using too strong an expression, demonstratively in proof, that the plague can be propagated by contagion?

What are the peculiar circumstances under which these cases occur? After an exemption from plague for three entire years, when the state of the public health was unsuspected, the disease, brought from a distance, is introduced into the lazaretto. There it is confined,—perfectly isolated,—and there, including the guardian, who got it on ship board, four persons from Constantinople contract it, whilst about 800,000 persons in the adjoining city and its suburbs along the shores of the Bosphorus remain exempt,—the persons attacked and the persons exempt, the small few and the vast many, alike situated as regards all appreciable circum-

stances, excepting in one particular, viz., that the latter were within the same enclosure as the imported cases of plague.

It would be tedious to discuss the matter farther. I fear I have already dwelt on it too long; if so, the importance of the conclusion in its bearings must be my excuse. The facts, as I believe them, are few and simple; they impressed my mind strongly, previously sceptical on the question of the contagion of plague, and, if I do not overrate them, they are well adapted to impress, in like manner, other minds, should there be others similarly in doubt. And, on such a subject, how difficult it is, indeed, to avoid doubt, especially after studying the works of some of the latest writers on it, as of M. Bulard and of Clot Bey, who have examined the disease not only in the same country, Egypt, but actually together in the same plague hospital, associated in the meritorious undertaking of investigating its nature, and who, notwithstanding, have made almost contradictory statements, and have arrived at contrary conclusions; the one persuaded that the disease is contagious, and the result of contagion, the other convinced that it is incapable of spreading by contagion, and that it owes its origin to local circumstances.

Collaterally, there are some points of interest connected with the main subject under consideration, to one only of which I now propose to advert. It is expressed in this question,—in what manner was the disease communicated to the four persons belonging to Constantinople, who contracted it in quarantine? Was it by contact with the plague patients who arrived from Alexandria, or in consequence of touching something belonging to them, or that had been in contact with them?

The information I have to offer is not so minute and precise as I could wish, and yet I hope it may not be altogether useless. From what I could learn, I have no doubt that the guardiano touched the patients, and assisted them in landing at the lazaretto. When he entered on the duty of attending them, he was exhorted to be on his guard, and to avoid, as much as possible, touching them. A Mahomedan, he had no fear, he said, of the disease, and he acted accordingly, taking no precautions against it. The porter, one of fifty employed in the lazaretto, all similarly employed, was chiefly occupied in removing the effects of the passengers, and the articles belonging to the vessel. I do not know that he touched any one of the sick. The son and daughter of the purveyor of the lazaretto, it is almost, if not perfectly certain, never came near the plague patients, for they occupied different parts of the building, and were kept apart equally by the rules of the establishment and their dread of the disease, being Franks, and, in common with all Franks of their class, holding the disease to be highly contagious. Were I to offer an

opinion as regards what I consider probable, it would be, that the young man got the disease by touching some plate, or glass, or piece of money, brought from a plague patient, and just before touched by him; and that the sister, who sickened after him, contracted the disease from him. He was employed under his father in supplying provisions to all who were in quarantine. How the monk got the disease, is extremely uncertain. He stated that he believed he owed it to the touching of a piece of paper, on which some salt was brought, supplied by the young man on the day that he sickened.

In conclusion, I would beg to revert for a moment to the method of inquiry which I have at present adopted. Its value in part, like that of every other mode of inquiry, more or less, must rest on the consistency of nature, *i. e.* on the series of events constituting human knowledge, occurring in certain order and constancy, thereby admitting of calculation,—a constancy expected generally in the functions of animals and vegetables, not less than in the revolving motions in space of the great masses of matter, and expected also in the phenomena of diseases, *i. e.* their symptoms, and in the organic changes which may usher them in, and in which they may terminate. Were there no such constancy, there could be no certain knowledge, no science; experience would be without value, experiment useless. Applied to the present subject, believing it not to be an exception, may we not fairly infer, if a disease can be proved in *one* instance to have been propagated by contagion, that this quality of propagation essentially belongs to it, whether efficient, like the function of generation in the untamed elephant, or inefficient, as this function commonly is in the tamed one; whether active, like the seed in process of germination, under the influence of moisture, warmth, and atmospheric air, or inert, like the same seed, excluded from the influence of these agencies. If this be granted, if it be generally admitted, a certain step in the inquiry is made, though it be considered merely a first step. Other questions will necessarily arise, and should lead to farther research, as the character of the contagious matter, whether capable of being formed like a proximate vegetable principle, or of being generated only like an animal species; what the circumstances are which conduce to its activity; what others are incompatible with it; with many more, which it would be useless here to enumerate.

The doctrine of consistency, in regard to pathology, I fear, it must be confessed, has been too little regarded. What vague complicated disputes have been carried on relative to the origin and nature, not only of the disease under consideration, but of many more, especially cholera of late years, and yellow fever! How little that is certain and well established, amidst conflicting

evidences, can be selected respecting either ! Is not this owing not only to the great difficulty of the subject matter of inquiry, combined with necessity for prompt practical decision and exertion on the outbreak of such destructive diseases, but also, and in no small part, to the common mode followed in conducting the investigation, not in an humble way, cautiously making sure of particulars, but, more ambitiously and boldly, by the first more attractive, and, at first view, more promising method to which I have alluded in the commencement of this paper. I would fain hope, were the humbler and safer method followed, as opportunities offer, even putting aside aid from experiments, which it might be very desirable and proper to make under certain circumstances, that much, which is now obscure, might be made clear; we might have it in our power to distinguish between contagious epidemic and simply epidemic diseases; and medical science might advance progressively, without stop, like any of the mixed sciences, founded on observation and experiment, and be almost equally precise in its details and its doctrines. And what has already been done in some of its branches is amply encouraging, especially in the instance of the exanthemata, with which we are most familiar, and also in the instance of thoracic complaints, the knowledge of which, during a very few years, has been so remarkably extended and improved through the new means of observation and scrutiny which have been applied to investigate them.

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ART. III.—*On the Treatment of Tropical Dysentery, by means of Enemata of Tepid Water.* By E. HARE, Esq., Assistant-Surgeon, 7th Irregular Cavalry, Bengal.

THE first portion of the following paper was originally printed at Delhi, in November 1847, for limited circulation among the medical officers of the Bengal army. I had for five years previously treated the men of my own regiment with such marked success, that, during this long time, I had not, as the returns of my hospital to the Medical Board can prove, lost one case from diarrhoea or dysentery, which are usually so fatal in India. At the urgent request of my commanding officer, I published, with extreme diffidence, the following paper, as a few rough hints to my medical brethren. The general favour with which it has been received, and the very numerous successful cases from others which have since monthly appeared in the Calcutta Medical Journal, and the official approval and recommendation of the Medical Board to the Government of Bengal, that I should be allowed to try the plan on an

extended scale in the large Calcutta hospitals, induce me to hope that I have not added one more to the numerous plausible but false theories of the day.

I have now, in the paper which continues this, and which forms Part Second, more fully followed out the details of my system, showing its adaptation to all the varieties and complicated forms of the disease. If this latter paper possess no other merit, it at least deserves approbation, as the first attempt which has yet been made, to account, by one simple cause, for all the remarkable facts which have been observed in the morbid anatomy of dysentery, which the observers have long wondered at, without attempting to explain.

It will be remarked, that, if this theory be considered proved, the treatment of dysentery, by injections, must follow as a necessary corollary. In both papers, I have most carefully avoided resting my proofs on any facts or cases of my own. This I have done purposely, because I know the suspicion with which these are always viewed, when coming from the author of new opinions. Every fact, therefore, is supported by the testimony of all the most received and experienced writers on the disease, who had not my theory to support;—that, by the mouth of two or three witnesses, every word may be established.

## PART I.

### COMMON UNCOMPLICATED ACUTE DYSENTERY.

Dr Annesley's description of Dysentery is as follows:—

“The view\* we have given of disorders, which depend on accumulations of morbid secretions and local matters in the cavity of the larger bowels, shows in a very remarkable manner, one of the very earliest states of dysentery. Collections of excrementitious matters tend very evidently to irritate and inflame the mucous surface on which they lodge, and cause ulceration and even sphacelation in a very short period, if neglected, or injudiciously treated. In a great many cases, dysentery is preceded by costive bowels, often of long duration. In a few instances, when the evacuations are copious, the diarrhoea subsides, and the patient escapes, at least for that time, a true dysenteric attack. This result seems to arise from the irritation produced in the bowels by the fæcal accumulations having subsided, in consequence of the irritating matters having been removed. Frequently the dysenteric symptoms are present from the first, the stools being scanty, streaked with blood, and with abdominal pain and tenesmus. In cases of this nature, the increased action of the muscular coats of

\* Vol. ii. p. 152.



the bowel, to which may be added the swollen state of the mucous membrane, especially about the sigmoid flexure and rectum, prevents the passage of the fæcal collections through their canal, and in many cases occasions complete obstruction, little passing away, but the perfect fluid secretions. In cases of this description, if the disease be not early subdued by very decided treatment, sloughing of the mucous coat takes place, followed by involuntary motions, when the fæcal accumulations at last come away, such parts of them at least, as have been dissolved, being washed off by the watery secretions poured from the irritated vessels of the inflamed surface."

This is the result of Dr Annesley's large experience, and in most expressive language he shows the fatal effects of accumulations of fæces and acrid secretions in dysentery, and that the danger of this disease arises solely from their retention. This is corroborated by Dr Gregory, Bampfield, and a host of others in equally large practice. The testimony of Dr Gregory is given in the following passage:—

"But\* the employment of purgatives constitutes the most important part of the cure of dysentery. They must be steadily persisted in, till fæcal evacuations have been procured, and that sensation of load in the bowels completely removed, which leads to the effort of straining, and then, and not till then, may the practitioner desist from the free use of his cathartics. In hot climates, the exhibition of mercury to produce salivation has been recommended, as an effectual method of putting a check to the advances of dysentery; but we have no reason to believe, that a vigorous and well-regulated employment of bleeding and purging is less efficacious in a hot climate than our own."

No one, I think, in the present day, trusts to salivation. On this point, Dr Macgregor's own case in his late work, proving the inutility of salivation, may be consulted.

Dr Raleigh says, that† purgatives are most requisite; but urges the use of those of an unirritating nature. But here is the difficulty. We can see the strong objection to giving large doses of irritating purgatives in a raw and ulcerating state of the bowels, and yet, from the dreadful results of delay, and the obstinacy of the constipation, see what fearful doses are constantly necessary. The 20 grain doses of calomel, and 3 or 4 ounces of black dose, given by Annesley, night and morning for weeks together, would excoriate most of our healthy bowels; how must it then injure and hasten to ulceration the inflamed intestines, already writhing in agony? Yet it *must* be given; for unless the hardened fæces and irritating secretions are rendered fluid and expelled, death must result.

The following is the testimony given by Bampfield (p. 118), the most practical writer we have:—

\* Practice of Physic, 1839, p. 550.

† On Dysentery, p. 75.

"Let it be remembered, that *however great* the quantity given, purgatives *must* be continued till their purgative effects be produced; and in all cases this should be accomplished in 12, or, at farthest, 24 hours (my plan does it in 10 minutes, and with a little soothing warm water); for the obstinacy of the constipation generally bears its proportion to the severity and danger of the disease; that is, he says, the more inflamed the intestine, the more necessity to irritate it with repeated excoriating purgatives." Alas! what a fatal necessity. "It may surprise," he goes on to say, "many of my readers, to learn that a purgative result is as difficult of attainment, in some cases of tropical dysentery, as in painter's colic. By perseverance, and yet disappointment, in the use of cathartics for many hours, the patience of the sufferer may be almost exhausted, and the resolution of the practitioner to persist may waver, but purgatives must *never* be abandoned; he must pertinaciously persevere in their use, till the intestines be emptied. I should not, he says, insist so strongly on the necessity of resolute activity, had I not observed the fatal effects of an indecisive and temporizing practice. I used to prescribe 8 to 12 grains of calomel, twice or thrice a day, till it salivated and purged, to which, however, from its uncertain operations on the bowels, I soon added 30 grains of jalap, or an ounce of salts. These with mercurial frictions generally purged; but one day I met with a case in which they did not purge till the third day, and the patient died on the fifth; thus announcing the danger of this indecisive practice."

Thirty-six grains of calomel and 30 of jalap daily, must, I think, be allowed to be a tolerably decisive dose.

"In 1806," he says, "three more men fell a sacrifice to this indecisive treatment; as its purgative effect did not ensue till the third or fourth day, and ptyalism could not be excited. Since that period I have given cathartics in the following manner."

And then he tells us what he gave in his improved practice. These are two cases, p. 122:—

"Angus Graham took 110 grains of calomel, 180 grains of jalap, 10 ounces of Epsom salts with infusion of senna to match, and 8 grains of ipecacuanha. All this in 36 hours, when he passed only one faecal stool."

R.—"Robert Allen took in 36 hours 90 grains of calomel, 132 of jalap, 120 of aloes, 140 of extract colocynth, an emetic, and an ounce of castor oil,—(and as if this were not enough, next day he took)—20 grains of colocynth and jalap, 10 of calomel, and an ounce of castor oil."

I wonder if Robert Allen and Angus Graham were ever healthy men after such discipline as this; and yet, no doubt, they would have died without it, or some substitute.

He allowed no truce either, after this first grand onslaught.

"Small pieces," he says, "of fœtid fæces, not larger than peas, get enveloped in the mucus of the intestines, and are retained, notwithstanding the fæcal evacuations are frequent. An assiduous renewal, therefore, of cathartics is necessary, until the intestine becomes quite easy."\*

Dr James Johnson, who trusted everything to 60 and 80 grains of calomel daily, says, that no doubt purgatives will cure if they act; but the difficulty is to make them act, till ulceration has begun, and it is too late.†

Thus, then, in this unanimous consent, that to empty the bowels is the first and last necessity in the treatment of dysentery, and that if it be not done at all risks, the patient dies; is it not truly wonderful, that a discovery in 1822, safe, as it is simple and harmless, and which gives the perfect command of the whole diseased part in dysentery (the large intestine), as much as if it were open, as the nostril or the mouth, should be unemployed and unheard of in India, where we are yearly losing hundreds by this fearful scourge? Dr Strachan, Inspector of Hospitals, Bombay, allows, that when "once a patient is seriously affected with dysentery, he is seldom perfectly restored again to health."‡

Dr O'Beirne in 1822 discovered, that passing the elastic tube of a stomach pump, above the sigmoid flexure, enables us with certainty to clear the intestines of their contents, by the simple use of a little hot water, and that too at once, without waiting three days for the intestine to ulcerate, before the drastic purgatives can soften, and bring away the fæcal matters; not to mention the mild soothing nature of the solvent, compared with heaps of calomel, jalap, and black draughts. Dr O'Beirne not only proves this, in its application to other diseases, but gives these two cases (which I abridge) as examples of its use in dysentery.

1. "A girl, aged 25, after wet feet, seized with rigors, pains and cramps in the belly, and incessant desire to go to stool, without passing anything but gelatinous mucus and blood, continues thus for eleven days till admitted into hospital, when her countenance was pale, expressive of acute pain, pulse 96, small and weak, tongue white and furred, skin cold and dry, nausea, pain on pressure of abdomen, severe and constant tormina, and tenesmus. She has passed nothing from her bowels but blood and mucus since the attack (11 days). The rectum was prolapsed, and in an inflamed state. The elastic tube was, without delay, passed up the rectum 9 inches; when a considerable quantity of gas escaped, and gave some relief. An injection was now thrown up, and brought away a quantity of blood, mucus, and scybala, ac-

\* P. 125. † Pp. 375 and 360. ‡ Annealey's Appendix, Vol. ii., p. 41.

accompanied by loud bursts of flatus; in a few minutes she declared she was quite relieved, and free from all pain and uneasiness. Next day she had slept well, and was free from pain and griping, but complained of a sense of uneasiness in the left iliac region. The tube was passed again, and an injection brought away a quantity of flatus, and solid and fluid fæces. She says she feels quite well. Two more injections cured in four days without a particle of medicine by mouth."

2.—"A girl, aged 30, pulse 120 and small, has retention of urine. The left iliac region tender under pressure. Ill since three days; has passed only mucus, blood, and a few scybala. Her bowels were constipated for nine days previously to the attack, which came on with shivering, followed by tormina and tenesmus. The tube was passed without difficulty nine inches with escape of offensive flatus. An injection returned without fæcal matter. Considering that this circumstance indicated an accumulation of fæces in the cæcum, and expecting that the escape of gas would cause the fæces to pass on to the sigmoid flexure, I waited on for a quarter of an hour, and then threw up an injection, double in quantity (four pints). In a few minutes the bowels discharged a large quantity of flatus, mucus, and hardened fæces. The pain ceased, and the pulse fell. She slept well, and the tormina and tenesmus ceased."

One more injection next day cured her. These cases are put at the end of Dr O'Beirne's book, which is filled with the application of his method to other diseases more common in England. I do not wonder that the method has been neglected; for dysentery there is rare. Dr Watson confesses, in his admirable Lectures, that his experience of the disease in England is so small, that his opinions are drawn from others (Bampffield, &c.) But this mode of treatment ought to have been noticed and applied in India, where dysentery is such a fearful scourge, and would have been, no doubt, but for the vagrant life which the army doctors lead, the difficulty of carrying books, and, therefore, caution in buying them. O'Beirne's is not a book on Indian diseases, and no wonder it has not been read here.

Bampffield did not use injections, "because," he says,\* "that they were rejected without fæces immediately they reached the rectum and lower part of the sigmoid flexure."

Annesley (p. 279) evidently likes injections, but never used more than twelve ounces, because the slightest distension of the rectum and sigmoid flexure expelled them again without fæces, only a little fluid matter.

"But even this," he says, "is an object worth attaining, and should not be neglected."

\* P. 126.

By passing the elastic tube above the sigmoid flexure, you can inject from four to six pints, and thoroughly wash out the intestine from cæcum to anus.

It is satisfactorily proved in the work of Dr O'Beirne, that the chief obstruction to the fæces always is in the sigmoid flexure, the rectum and itself remaining almost empty both in health and disease. The fæces accumulate, therefore, above the sigmoid flexure, the twistings of which, narrowed as it is in dysentery by spasm, and the swollen state of the mucous membrane, form so complete an obstruction, that nothing but fluid, and that with difficulty, can pass. The accumulation itself, too, greatly increases its own obstruction, by impeding the circulation of the mucous membrane below it, almost to strangulation, and causing sometimes so much swelling, that the mucous membrane protrudes through the anus, causing prolapsus. Now, Bampffield and Annesley, with their syringe, had no means of injecting more than the rectum, and a little, perhaps, of the sigmoid flexure; but they could never reach, through the obstructed sigmoid, the fæces beyond. The irritable rectum, too, on the slightest distension, contracts on and expels the injection. Even in health, if a small quantity of fæces pass into the rectum, it produces an instant desire to stool; but large quantities may accumulate for weeks above it without causing this sensation.

If, then, a tube can be passed directly into the fæcal mass, above the sigmoid flexure, this feeling, on injecting, will not be immediately felt, and the water, by the pumping action, being intimately mixed with the fæces, it is evident that the injection cannot return without fæcal matter with it. It is astonishing how readily a hard mass of fæces will soften and break down in water. Any one may prove this for himself without much trouble. The tube, too, can be moved up and down, and the water made to wash its way, at all points of the descending colon. By the first injection, some fæcal matter, at least, will be softened and removed, and the injection can be applied again and again, till the relieved intestine eject itself the harder matters, reduced, as they must be, in size, if any remain; and this, in fact, is the way in which purgatives evacuate the fæces in dysentery, having first reduced them to a soft or watery consistence by the fluid secretions they elicit from the intestines; but what increased irritation must they cause in doing this.

Raleigh,\* although he recommends purging to carry off acrid secretions, makes the following statement:—

“That scybala and solid fæcal matter are never found in the intestines of persons labouring under acute dysentery in this country.”

\* P. 75.

Johnson, on the other hand, informs us that,\*

“By bleeding, intestinal strictures are relaxed, and scybala and fæcal accumulations pass off; and that if a patient die at a less advanced period of the disease, or where mortification has not relaxed all stricture, fæcal accumulations are found in the colon above these strictures.”

I need not repeat the representations of Annesley and Bampffield, and, I may add, every other writer but Raleigh. These men all practised in the East Indies, and in Calcutta too. Bampffield, p. 54, informs his readers:—

“I have treated dysentery in many parts of Europe, America, Africa, West Indies, China, and the Pacific Islands, and have not been able to discover *any* essential difference between dysentery in these countries and in the East Indies.”

In Annesley's cases, large fæcal stools, and constantly scybala were voided before his eyes. He kept all the close stools by the bed-sides purposely to examine them daily. He was a little too careful, perhaps, in this matter. But read not the theory but the practice, the actual cases of Bampffield and Annesley. I select one from a multitude. Case (174) headed “very acute dysentery, *from* fæcal accumulations.”

“At first, purgatives brought away large fæculent stools; but watery evacuations continuing, Dr A. supposed that there was still lodgement of fæculent matter somewhere in the intestines, and that the watery evacuations were the effect of the irritation caused by its acrid qualities. Large doses of purgatives were therefore continued for several days, but no fæces came away. On his death, the small intestines were found loaded with fæculent matter. The purgatives had brought copiously away at first the fæces from the large intestines, so that, before this evacuation, the man's abdomen was crammed with fæces, both large and small intestines.”

Can there be a mistake here? And this was in India. But why multiply similar examples which may be found, not assertions, but *facts, cases*, in every writer on dysentery, from Galen till now. I cannot account for Dr Raleigh's assertion, and it is vain to try; but I think, that where the accumulations are not very large, it is probable that nature herself, by throwing out fluid secretions, softens down and evacuates them; but in doing this, the gut is ulcerated and destroyed, though on dissection it is found empty. Dr Annesley says, p. 163:—

“The reason why scybala and solid fæces are seldom found in East Indian dysentery, is that the serous fluid copiously secreted from the irritated intestine washes it, squeezed, as it were, through the constricted canal in a liquid form.”

\* P. 350 and 351.

These views, as to the retention of the excremental matter in dysentery, are elucidated and confirmed by the case of Dr James Johnson\* in Calcutta. Nothing relieved the symptoms till the calomel which he took procured a copious *fæculent* bilious stool, succeeded, he says, "by such delightful feelings, that I prayed aloud." How then can Raleigh assert, that solid *fæcal* matters are *never* found in the acute dysentery of this country? I suspect that in almost all that die, the efforts of nature have already softened and carried off in fluid the solid matters, and that Dr R. would have got rid of these in the solid form, and saved often the lives of his patients, had he given purgatives more freely than he did; for he gives very small doses compared with others. If dysentery attack a man with costive bowels, (and one-third of Europeans in this country have very costive bowels), there *must* be *fæcal* matter, and very often scybala in the intestines, which, from the nature of dysentery, cannot be expelled without nature's most painful efforts. I do not assert that dysentery is caused by accumulations; I think it rarely is. But when caused, the *fæces* it finds in the intestine, and the irritating secretions it afterwards adds, are the principal, perhaps *only* cause, of its destructive termination, and I have no doubt that the danger of dysentery is essentially caused by the long contact of *fæces* and acrid secretions with the inflamed surface of the intestine; and if these be freely removed, the danger at once ceases.

No author except Raleigh says that solid *fæces* are never found; and any one who doubts that they are not almost *always* found, and in large quantity too, had better consult Annesley's plates of dysentery, which are not selected to illustrate my theory. He will find them *all*, and without exception, plates of *fæcal* accumulations, sometimes in large lumps here and there (see† figs. 3-6). Sometimes the intestine is so crammed, that (fig. 2) the horizontal colon is bent by its own weight, down from the stomach to the region of the bladder. Now this proves the point; for Annesley, with far larger experience than Raleigh, sketched these from nature, as fair specimens taken from a multitude of similar cases of the disease (dysentery) as he saw it with his own eyes; and Bampffield, with all others but Raleigh, says the same. But even if Raleigh be right, that nothing but fluid matters are found in the intestine, as I will show hereafter, it increases the value of the treatment by injections.

There is another circumstance which, on examining these plates of Annesley, must strike every one, viz., the manifest impossibility, in these bad cases, of ever overcoming the contractions and emptying the bowels by purgatives, or by calomel even to salivation, and the beautiful adaptation of water, to stretch these con-

\* P. 368.

† See the plate copied from Annesley at the end of the paper.

tractions by its soft equable expansion, and dissolve and bring away the solid lumps behind them. Annesley's plates from nature are indeed well worthy careful study; I wish for no other proof of the value of injections. It is perfect.

That *something* fluid or solid, whichever you like, is delayed in the intestine by the swollen mucous membrane, causing great distress and irritation, is manifest, or why have we invented two new words, "tormina" and "tenesmus," to describe the painful strivings of nature to pass *something* which it cannot? Who ever read a case of dysentery where griping tormina and tenesmus are not always selected as the only terms which could properly express the symptoms? Now what do these mean? why tenesmus from *teneo* to *hold fast*, and tormina, the coiling, twisting, writhing of the intestine, as in strangulated hernia, to overcome the *holding fast*.\* But see what a blessed means we have in hot water, of gently expanding this obstruction, and allowing the acrid fluids to escape, which nature is thus painfully trying to force through; or if it be solid matter, of dissolving and clearing it out. That fluids may be, and are detained, is evident; for, in the work of Dr O'Beirne throughout, and the two cases quoted above from it, it will be seen that even gas (flatus) is detained in large quantities, but instantly evacuated by the flexible tube. Therefore whatever it be, solid, fluid, or gas, empty it out forthwith, and let the intestine rest; nature will complete the cure.

I have thus shown that all writers on dysentery tried their best to empty the bowels by purgatives, as the one grand object in its treatment; for, this not accomplished, vain are their efforts to subdue the inflammation. Suppose an eye inflamed from cold or otherwise, put a lump of dysenteric fæces within the lid, and shut it up there, do you think that any bleeding, or 20 grain doses of calomel, will prevent ulceration of the coats? This may be a coarse argument, but it is true, till it can be proved that one part of the great mucous membrane, continued in one piece from the eye to the anus, differs anywhere in its nature. What would you do in such a case? why, wash out the filth first, and then the inflammation will subside of itself, or with very moderate depletion.

Nature has given us, in the eye, an example of all the important structures of the body,—the mucous, fibrous, and serous membranes,—and she has, moreover, put a piece of glass (the cornea) there for us, that we may look through and see her wondrous working, like the glass side to a Sam Slick clock. Now, what are the effects of remedies in inflammation of these three membranes? We can look at them here, and there can be no

\* This is not the usually received derivation of Tenesmus, Τένεσμος, or Τένιςμος, which is derived from Τένω, I stretch. But we do not alter the author's statement.



mistake. Why this? Salivation and bleeding stop the inflammation of the serous lining of the iris and cornea, as if by magic; the moment the gums become tender, then stops the effusion of lymph. Salivation is here, therefore, our sheet anchor; it is a cure; we see it with our eyes. In inflammation of the fibrous structure, what does good? Why, colchicum, calomel, and opium, alkalies, &c., as in the rest of the body in rheumatism. But now for the mucous membrane, how of that? And it is most particularly exposed to our notice, being outside of all. Why, bleeding gives only temporary relief; you can empty the capillaries while the faintness lasts; but they will fill again if you bleed to death's door. Local depletion, in great moderation, is better; but the grand secret of treatment is, cleanse the eye with warm water from its own acrid secretions, and apply lotions of alum and nitrate of silver. See how the tears in ophthalmia scald the cheeks of a child; and if you add to these a lump of foetid fæces, what chance have you of curing? But, now, about salivation; what good does that do? Saunders, Lawrence, Guthrie, Mackenzie, Watson, all say none at all. Never try it. Now, why should salivation be a whit more successful in inflammation of mucous membranes anywhere else? Will it cure bronchitis? No one, in truth, says it is useful, except Dr James Johnson. Twining and Annesley, his disciples in other things, only give it as a purgative, and that was the way, in fact, in which it cured Dr Johnson. Read his ecstasies on going to stool; nothing relieved him till the one scruple doses of calomel which he took, procured "a copious *feculent* stool."\*

Calomel is, indeed, a very effectual purgative, and, when it salivates, opens such a flood of secretions, that it seldom fails to purge; but it is surely better to do this with warm water; for, first, it is no benefit to the constitution to be salivated; and, next, it sometimes takes so long before the calomel will either purge or salivate, that, when it at length does, the intestine is irretrievably ulcerated. In Dr Macgregor's remarkable case, the calomel salivated, but failed to purge,† and "all the symptoms continued unabated." Dr Macgregor's book is an admirable commentary on the numerous dreadful results produced by the system of salivating in dysentery, which is still the common routine in India.

#### CHRONIC DYSENTERY.

If injections are valuable in acute, still more are they in chronic dysentery. This disease Annesley describes in the following terms:—"The inflammation of the acute stage leaves the villous

\* P. 368.

† See his late work on Dysentery, &c., p. 95. Dr Macgregor, also, finding other purgatives inefficient, uses croton oil, in five drop doses, frequently repeated.

coat of the colon throughout hypertrophied. Its papillæ having an erect rough appearance and feel, of a dark red or purplish hue, with here and there livid patches of congestion, particularly at the caput coli, and transverse arch, where ulcers are often found. The sigmoid flexure is often very much contracted, its villous coat of unnatural thickness, and inflamed as well as the rectum. The lining membrane of the small bowels for some way up, is in much the same state, ash-coloured ulcers, &c. &c."

Now, I ask, is this a condition of the intestine, likely to be improved by the irritation of purgatives, and yet the contraction of the colon, particularly the sigmoid flexure [look\* at Annesley's plates of these contractions], together with the loss of muscular power and peristaltic action, necessarily succeeding the acute stage of the inflammation, render it absolutely necessary that they, or some substitute, should be given. The secretions, too, continue acrid, the appetite is often ravenous, while the powers of digestion are much weakened. The half-digested food and acrid secretions accumulate and distend the intestine above the contractions of the colon, causing dreadful irritation, and constant purging of fluid matters. Purgatives have, therefore, at all times been unwillingly resorted to, as a necessary evil; every writer inculcating in theory the use of the mildest laxatives, but in practice, in their *cases*, using from necessity calomel, Epsom salts, black draughts, and compound jalap powder, [Annesley, pp. 366, 367, 369.] To correct these, they used freely opiates and astringents, which, in checking the dangerous symptoms of the purging, produce accumulations. The accumulations require again purgatives; and so on the changes are rung, till the patient dies, or escapes only by the powers of a strong constitution; which, nevertheless, is often weakened, if not destroyed, by the previous treatment, for the acute stage.

That this is no exaggerated picture of the present treatment of chronic dysentery, any one who has been much in a European hospital in India, must acknowledge. See what Raleigh says, p. 117, of the wretched uncertainty of his treatment in chronic dysentery. I will illustrate it by extracts from Bamfield, who describes from nature, with all the ample experience of years in large hospitals:†—"This retention of fæces occurs in every variety of chronic dysentery, although the patient have frequent loose stools; and it probably takes place above the diseased portion of the intestine."‡ This state of retention is difficult to detect, for the patient at the time has frequent stools, which are not very scanty, and induce him to describe his complaint as a free and troublesome purging. A retention of fæces always causes gripes, flatulence, increased evacuations of morbid mucus and secretions, general fever, and nervous irritation, only to be reliev-

\* See a few of them copied at the end of this paper.

† P. 172.

‡ P. 184.

ed by a purgative. He acknowledges (pp. 197, 199) the necessity of using astringents, and their great success for a few days, but allows that they are too apt to produce constipation and its painful and disordered consequences. The constipating effects appeared to be more disadvantageous to the patient than the astringent powers on the morbidly secreting vessels were beneficial; and I found that, if too weak, they produced no evident effect;—if strong, they always produced constipation, and with it increased morbid secretions, and tormina, and sometimes actual relapse.\*

One cannot read Raleigh's cases (pp. 118–148) without being convinced that the reason of the various effect of the sulphate of copper, was owing to the same cause. Annesley says the same, (p. 371):—"It should always be kept in recollection, that the *necessary* (he says) employment of tonics, astringents, or anodynes, occasions accumulation of fæces, and requires purgatives. When this plan (astringents) is being employed, purgatives will be necessary, and no medicines can be more beneficial than a *black dose*." And yet he says (p. 376) "active cathartics excite the raw and inflamed surface of the constricted part, and increase the morbid state which it is our object to remove."

Again, p. 378,—"In cases of contraction of the colon, the chief object is to preserve the contents of the colon in a fluid state, to prevent accumulations of fæces from forming above the stricture, and the irritation they would occasion."

Thus the disease requires astringents to check it, astringents require purgatives, and purgatives aggravate the disease. Link within link of fatal necessity. See how dissatisfied Bampffield is under this destiny:—"It† must be granted that astringents display great powers over the morbidly secreting vessels; but it must also be admitted, that if they induce constipation, the morbid secretions are much increased, and it becomes necessary to abandon them. If the constipating effects of these astringents could be generally obviated, by the combination of a defined quantity of any purgative medicine, which would not decrease their astringent effects on the morbidly acting vessels, then it would appear, that the desideratum for the cure of this disease would be obtained."

Since 1822 Dr O'Beirne has discovered this desideratum; yet no one has thought of making use of the method, though it is infinitely preferable to this half purgative half astringent system, proposed here by Bampffield (if it were possible, which it is not). By the introduction of the elastic tube, you may wash out daily, with the most soothing applications, warm water, or milk and water, the excoriated intestine, removing its acrid secretions, and the fermenting, half-digested fæces, fomenting its tender surface the meanwhile, and softly stretching the strictured parts, with the gentle expansion of water. And when in the intestine, thus

\* P. 372.

† P. 199.

daily cleansed and soothed, the inflammation and irritation have been calmed, what numerous applications may be conveyed by the tube to the whole diseased surface of the colon, from the cœcum to the anus, gently to constringe the overstrained vessels, and heal the ulcers, the suggestion of the effect of these medicines, in disease of the mucous membrane of the eye, will be sufficient proof.

No constipation can be caused in this method of injecting, or even by giving these astringent medicines by the mouth, for the fæces ought to be daily removed by a large injection of water, previously to the exhibition of the small astringent injection, or astringent medicine by mouth, and great care ought to be taken, that a sufficiently large injection be daily thrown up to empty the bowels completely.

For proof of the rapidly curative effect on the strictures of removing the accumulations behind it, I need only mention the instantaneous cure of urethral stricture, by puncture of the bladder.

Thus, then, the intestine will be at once relieved from all sources of irritation, and remain quiet throughout, and this\* itself is a cure. But besides, we can apply any local application we please to its diseased surface, and strengthen the constitution too, which is the real healer, by giving tonics, and astringents by the mouth, uninterrupted by purgatives, and more than all, the liberal use of a nutritious diet instead of the milk and low diet now given; for we need not with injections fear the accumulation of undigested food in the intestines.

Bampfied says, p. 234:—"The healing of an ulceration of the intestine is a process of nature, carried on under such disadvantages (irritation of fæces, and constant purgatives), as will from every probability require a long time for its completion, even in the most favourable circumstances. It is indeed observed by Galen, that ulcerations of the intestines more readily admit of a cure, than of other internal organs, *because* medicines injected in the form of enemata become immediately applied to the affected parts, but it must be *evident*, says Bampfied, that this rule can *only* apply when the ulcer is within reach."

With his apparatus, only the rectum was within reach. How gladly, then, Bampfied would have used this invention, which places the whole colon perfectly so. The success which attended the small injections of lead, used as a styptic by Raleigh, in some hopeless cases of hæmorrhagic dysentery, ought to have induced him to apply this method on a larger scale, in more hopeful cases, where death was less certain. The pint and a half which he says will cover the colon of a dead subject, is quite insufficient for the living, where the peristaltic force is still acting. Three pints is

\* "Put the bowels in splints," as Broussais says.

a small, four pints a moderate injection, frequently five or six will be required. In a thin patient, you can easily trace the injection as it passes up the colon. Till three pints have been given, it does not pass the first angle of the transverse colon. If the patient then lie on his left side, it rapidly flows into and distends the cœcum. You cannot possibly inject too much if it is done slowly. The intestine, when full, will always react and expel its contents. See an admirable paper in Dr Corbyn's Journal, Nov. 1836 :—The author says, "I removed the colon, and found that it would hold eleven pints without threatening to burst at any part."

The living intestine is, of course, still stronger, but, in practice, three or four pints will almost always be found sufficient.

The astringent injections I have found most useful, have been nitrate of silver, fifteen grains, dissolved in two and a-half to three pints of water, a strong decoction of bark, and a mixture of catechu and chalk. The first and last I have found particularly useful. No doubt the nitrate of silver might be increased much in strength, but I have not been able to ascertain yet how much, from a dislike to try dangerous experiments without extreme caution. It is absolutely necessary to clear out the bowels first with a large injection, before injecting the medicinal one, which may be smaller.

I believe that inflammation of the mucous membrane of the colon ought to be treated on exactly the same principles as in the conjunctiva, where salivation is now rarely trusted to, but great use made of astringent lotions, with moderate depletion, and great attention to cleanliness, frequently washing away secretions by warm water or some mild lotion.

Free venesection will often be required in acute dysentery, and leeches in chronic, but very seldom, I think, mercury, though even this might be used as an ointment. It may be as well to observe, too, that it is by no means necessary, in all cases, to introduce the elastic tube far up the intestine; though really the inconvenience to the patient attending it, even in the worst cases, is so trifling, as to make it almost a matter of indifference, if carefully done. After the first clearing of the intestine, a small distance, four or five inches, will suffice, and, latterly, the common short rectum pipe, but the pump is indispensable; the common squirt will not do; and it is most requisite always to inject a full quantity, or we may fancy the intestine empty when it is not. A gentle mulling of the abdomen with the hand, while the injection is still in the bowels, assists it greatly in bringing away the accumulations. I generally keep the patient on his back, occasionally turning him to the left side. When he complains of, and I see that there is distension of the abdomen, I cease to inject,

and commence, if he can bear it, gentle mulling. The patient then sits up, and the injection never fails to carry all away with it.

In an hospital, where the labour of constant pumping might be laborious, a common mussuck,\* placed on a chair, and its mouth tied to the flexible tube passed up the bowel, will answer every purpose. A man must sit upon the mussuck, moving up and down with a jerking motion. This, if the mussuck is full, has even a greater injecting power than Reid's small pump, which, however, for hospital use, might be made much larger with advantage. The mussuck may be moved from bed to bed, and the injection given as easily as a dose of calomel or black dose. In children, a large-sized gum catheter may be passed; I have never seen, even in them, pain caused by it. But I must refer to O'Beirne's book for proof how easily and safely the elastic tube may be introduced in all states of the intestine; its applications are, indeed, innumerable; but I hope it will not be long ere its use in dysentery be general throughout India, and happy shall I be if this imperfect essay hasten so beneficial a result.

I cannot refrain from adding one case illustrating the above.† It was treated by Dr Brown, civil surgeon, Simla.

Lieutenant M——, 60th N.I., suffered from acute dysentery in September and October 1842, while on service with the Cabul army. In February 1843, he was seen by Dr Brown at Simla, and was then in a dying state, passing 8 or 10 motions daily, mixed with purulent matter and blood. Injections of nitrate of silver, 3 grs. to the oz., with a little opium, were ordered twice a-day, and were given with the common short pipe, 3 or 4 oz. in quantity each time. The medicine he took besides, was merely Twining's pill of blue pill, ipecacuanha, and opium.

The injections were commenced about three weeks previous to his death. After two days' use they seemed to give great relief, and the bowels became more quiet, so much so, that he never passed more than two, generally only one stool daily, except after an aperient, which was occasionally given. Notwithstanding this improvement, he continued to lose strength daily, till he sank and died towards the end of April. On examining the intestines after death, they were found thickly studded with small dark-coloured circular ulcers, from the cæcum to the end of the sigmoid flexure, to the point, in fact, where the sigmoid joins the first turn of the rectum into the pelvis. From this point to the anus the ulcers abruptly ceased, and the whole surface was thickly covered with as many *scars of ulcers quite healed*.

It appears, therefore, that as far as these small injections of nitrate of silver reached, they healed the ulcers, even under the

\* An Indian water bag.

† See Med. Register, August 1848, Calcutta, for this case.

great disadvantage of a broken constitution, three weeks only before death, and the extensive irritation of active ulceration throughout the rest of the intestine to the cœcum.

In spite of this, the ulcers healed wherever the lotion could reach them ; what reason have we, therefore, for doubting a similar result throughout the colon, if the injection be applied to it by the long tube ?

## PART II.

### COMPLICATED DYSENTERY.

That the two forms of dysentery above described are the most frequent, appears from the writings of all the most experienced authors on dysentery, who must be supposed to describe the disease as they saw and treated it,—and it is no rare disease. The beautiful plates, too, of Annesley open the abdomen for us, and present to our very eyesight the fæcal accumulations, and the fearful ulceration which they caused.

Surely Cullen chooses the most common form of the disease, when he defines dysentery as “frequent mucous and bloody stools, the fæculent matter of the bowel for the most part retained, (rentis plerumque fæcibus alvinis).”

Twining defines dysentery, the most common form as he saw it in Calcutta, “the natural evacuations at some stages, and often at a very early period of the disease, suppressed.” But I am often asked, “Granted that injections are the most rational treatment for this the pure form of dysentery, is it likewise equally adapted to those forms complicated with fever and liver disease so deadly in Burmah, Chusan, Walcheren, and often in Bengal?” To answer this question is the object of this second paper.

Dysentery has many causes. The most frequent seems acknowledged to be, a sudden check to the perspiration at the close of the rains, when the body has become morbidly accustomed to that copious excretion during the previous hot weather. This produces a revulsion or congestion and inflammatory action in the intestines, which are thus suddenly called upon to perform the whole excreting duty of the skin as well as their own. The obvious treatment in this case is to restore without delay the secretion from the skin by warm baths and bleeding, having first cleansed the intestines from all the irritating matter which is causing griping and tenesmus, and which irritation, if continued, will counteract your attempts to relax the febrile constriction of the skin by bleeding and hot baths ; but having done this, the bath and the bleeding will readily act ; the patient will be placed at ease in the space of an hour ; and, if there still remain any irritation, a dose of Dover’s powder or opium completes what still remains for the cure. In a

few hours after, the bowels ought to be again emptied, and perhaps daily, for a few days' continuance, injections ought to be used till the bowel recovers its tone. It may be mentioned, too, that hot water injections are the most powerful diaphoretic we possess.

Another very common cause of dysentery is drinking bad water. This was a frequent cause of the dysentery in Affghanistan, and it is so also in some of our hill stations, Simla, Kassowlie, &c.

Now, while a purgative is daily taken with the food, you can scarcely hope, by any means, to stop the purging it produces; but purify the water by distillation, or filter\* rain water, and keep the intestine empty and at rest by injections and opium, and the disease will cease as quickly as the inflammation of a removed blister by poultices and opiate lotions. But even if you cannot remove the cause, viz. the water, still you may do great things, till the patient becomes accustomed to it, in preventing its injurious effects on the intestine by keeping it empty, and thus as free as possible from the irritant, and by controlling the irritation which remains with catechu, chalk, and opium, either by the mouth or rectum. By the rectum best, for the opium has thus as much or more soothing power on the bowel, and less injurious effect on the appetite and head. These astringents, as now given, require to be combined with alternate purgatives, thereby increasing the very irritation we are called upon to cure.

To dysentery from bad food the same remarks apply, but note these two causes are daily continued ones, and therefore safety from relapse cannot be expected till they are removed.

Another frequent cause of dysentery is disease of the liver, which if it be chronic will be a continued cause. It has been long known that disease of the heart, causing any obstruction to, and, therefore, congestion in, the blood vessels of the lungs, renders the patient liable to constant attacks of catarrh, and inflammation of its mucous membrane. In the same way, any obstruction in the circulation of the liver, as congested and cirrhotic or contracted liver, must congest the whole portal system behind it, and render the mucous membrane of the intestines equally liable to attacks of inflammation or dysentery on the slightest causes.†

\* Filtering the common water is not sufficient; all the impurities held in *solution* pass through unchanged; those *suspended* in it alone are removed.

† These causes,—checked perspiration, indigestion, &c.,—we must of course diligently seek for, and modify our treatment accordingly; but do what we will, except we can cure the organic disease of the liver, the dysentery must recur again and again; and as our treatment of it must thus be often renewed, and our patient's constitution is already injured by his organic disease, we must be particularly careful not to be too active in our remedies or depletion; and it appears to me, that injections are peculiarly adapted to these cases, for they palliate the symptoms for a time, and save the bowel from ulceration, till the powers of nature can equalise the circulation, and restore the system to its former equilibrium.



Dr Watson says (p. 474), " Intermittent fever causes dysentery, by congesting the spleen and liver, and through them obstructing the blood in the bowels,—upon this, congestion and inflammation may readily supervene. Hence dysentery and hepatic disease are often united." Now an abscess may form in the liver during this its congestion and inflammation, and thus exist at the same time with the dysentery, without being caused by it (the dysentery), but by its own congestion and inflammation. But all modern pathologists are agreed, that this is not the most frequent cause of abscess when united with dysentery, but that the liver abscess is subsequent to, and the effect of, the dysentery, either by the absorption from the intestine of acrid matter, causing inflammation in the liver, and subsequent abscess, or by the direct deposit in its substance of pus, generated in the inflamed veins, ramifying on the inner surface of the colon. Dr Watson (p. 16) says, that " Hepatic inflammation and abscess are caused by the veins carrying pus and acrid matter from the intestine. A writer in the *British and Foreign Review*\* (p. 359), argues, " Though it will not account for all abscesses, yet that abscess of the liver occasionally, and indeed often arises, from phlebitis and purulent absorption, is an undoubted fact." Annesley, and Geddes (p. 311), and many more if necessary of the best authorities, might be quoted. The very road the poison travels may be traced after death. The same writer (p. 344) states, " We have witnessed several times, in dysentery, lymph evidently effused over the course of the larger vessels of the mucous membrane." Parkes,† after most numerous dissections, asserts " That the mesenteric glands are always enlarged and inflamed." Again the facts are, he says (p. 117), " The intimate connection of dysentery and abscess, which is undoubted, and the universality of affection of the colonic glands in secondary hepatic abscess." Twining (p. 63) is still more particular in his description, thus.—" The glands of the mesentery and mesocolon are often enlarged, sometimes inflamed, and more rarely suppurating, and the portion of the intestine corresponding to the situation of the affected glands usually contains a deep and large ulcer," just as in a syphilitic ulcer on the penis, or a dissecting puncture of the finger, the glands running up from it are the first to inflame. Some few assert that this is only the effect of irritation, and yet we know that the system is contaminated from the ulcer on the penis, and we can prove that the poison travels this road too; for matter taken from the enlarged gland, will produce, by inoculation, syphilis in others, and excite a syphilitic sore in the patient himself, if applied to a puncture in another part of his body. Twining thus describes the worst cases (p. 60):—" In three or four days (after the attack) the stools have a horrid odour, resembling putrid

\* October 1847.

† Johnson's Review, p. 373. October 1846.

blood. It has been compared to the smell of putrid flesh in an anatomist's macerating tub; and there is a foetid cadaverous exhalation from the patient's body." This foetid secretion from the skin surely proves the fact of the absorption of the filth from the bowel. Is not the colon, in these cases, a large partially open abscess, full of the foulest contents; and in treating these when external, do we not instantly syringe them out freely with water, and diligently inject solutions of chloride of lime and soda? Read Dr Watson, p. 760, where he treated successfully, by these means, abscesses of the throat, attended with putrid symptoms, from the absorption of pus in scarlet fever. Druitt remarks (p. 56), "If the abscess is very large, or if, after the admission of air, the pus have not free exit, a most serious train of consequences will ensue. The pus exposed to the air putrifies, the hydrosulphate of ammonia, the product of putrefaction, is absorbed, and may be detected in the blood and urine. The interior of the sac inflames chiefly from the putrid pus, and then the grave and irreparable local disease, together with the contamination of the blood, induce typhoid fever, under which the patient sinks." Again (p. 307), after showing that in phlebitis abscesses form in the liver, and that the hepatic veins are sometimes found filled with lymph, he says, "That these consecutive inflammations and suppurations are really caused by the admixture of poisonous secretions with the blood, will be further evident, when it is considered that they are liable to occur, not only after phlebitis, but also after the supuration in the cancelli of bone, and cases where an imperfectly contracted womb becomes filled after delivery with half putrid secretions, in all of which, there is a ready access for poisonous matter into the blood."

It thus appears that there are two dangers in allowing the secretions to remain in the colon; one, that its veins in contact with them may become inflamed, and deposit their pus in the liver;\* and the other, that the secretions themselves may be absorbed, and contaminate the whole body. It is well known that puerperal fever, caused by this absorption of putrid matter, is highly contagious, and the excretions by the skin, breath, &c., from the corrupted blood, will reproduce readily the same disease, uterine phlebitis, in others predisposed. Now, Harty has satisfactorily proved, that dysentery, when united with typhus, becomes contagious also; and the writer in the British and Foreign Review,† taking his

\* The veins of the uterus empty themselves direct into the *vena cava*, and therefore the pus is mixed up at once in the general circulation of the heart. The blood from the intestines passes *first* through the straining substance of the liver. Thus, consecutive abscesses all over the body, as in uterine phlebitis, are seldom found in dysentery. In like manner, quicksilver injected into the blood of the *vena porta*, was separated from the blood and deposited in globules in the substance of the liver.—See Watson's Lectures.

† Oct. 1847.

own examples, shows, that this fever was not always pure typhus, but had frequently a malarious origin, so that a disease such as malarious fever and inflammation of the womb, both not in their own nature contagious, may become so, when the blood has been contaminated by the pus and secretions they produce; the poison of the inflamed uterus, reproducing in others uterine phlebitis, and that of dysentery re-exciting dysentery; and the reviewer says on this point, p. 344, "Our space will not permit us to enter into it; but before finally leaving it, we wish to direct inquirers to a phenomenon which has not been sufficiently studied. We have witnessed several times in dysentery, when there was diffuse sloughing, lymph on the mucous membrane evidently effused over the course of the larger vessels. We would recommend attention to the condition of the larger vessels in these and similar cases. It may be, that the state either of the vessels or of the blood, has an effect on the type of the inflammation." He here asserts the anatomical fact, that the veins are inflamed in these cases, and states his belief that the typhoid symptoms accompanying depend on their inflammation. How contagious the secretions are in these cases, may be learned from p. 355, where he says that Dr Morton was twice attacked with dysentery, from incautiously inspecting the stools of a patient; and (p. 357) Pringle relates, that, when the troops left a village, which had been converted into an hospital, and which was filled with the dirty straw covered with the dysenteric excretions, the disease immediately ceased to be propagated. Harty asserts (p. 260), that the necessary, and even the clyster-pipe, used by the dysenteric patients, propagated the disease to others. Pringle\* again:—"When mortification takes place, the distemper is most contagious, whether in producing a simple dysentery, or one combined with fever." So that the effect increases with the putridity of the cause.

This form of dysentery, viz. complicated, with malarious fever and typhus, is the pest which destroyed our armies in Walcheren, the Peninsula, Burmah, and China, and which Sydenham describes in London, where from 1667 to 1692 it destroyed its 2000† victims yearly. Dysentery complicated with only pure remittent or intermittent fever, as we see it in Bengal, China, and Burmah, is perhaps never contagious; but, according to the best authorities, it may become so, when it assumes the typhoid type, from contamination of the blood by absorption of poison into the circulation. In the malarious cases, of course, we must administer quinine largely, to assist our local treatment of the bowels. Also, if it be pure typhus fever which accompanies the dysentery, we can save the bowel from destruction, and stop fresh absorption

\* Johnson's Review, July 1847, p. 81.

† Heberden on the Increase and Decrease of Disease, 1801.

and contamination of the system by injections; while we give wine, ammonia, and support, adopting the usual treatment of palliating symptoms, which is found so beneficial in typhus. And by thus soothing the bowel, do you not remove the constitutional irritation, reacting on the fever, and save your patient from death, by palliating its most dangerous symptom? Dr Ferguson, in his Lectures, always recommended this plan as the only treatment for puerperal fever, viz., to cleanse the uterus well from its foul contents, and thereby prevent fresh absorption and contamination, while you treat the typhoid fever remaining by the usual expectant method.

I am far from believing that these complicated forms of dysentery can, if severe, be saved by any method of treatment; but I can safely assert that injections will save the lives of hundreds of the slighter cases who now perish under our present system, and a large proportion, too, of the severer, if seen *early*, before much injury has been sustained; and does not this plan overcome the difficulty which Dr Wilson\* complains of in China, when he says, p. 53, "With very few exceptions, the periodic fevers and dysentery have been complicated with each other, and this complication has made a principal difficulty in adjusting the treatment, for that promising utility in the fever has generally proved injurious in the flux; the flux and fever constantly alternating till the subject is worn out; the exhaustion being generally associated with an immense amount of organic injury"?

There is one variety more of complicated dysentery,—the scorbutic; and a scorbutic taint has often been mentioned, as associated with the dysenteries of Calcutta, Burmah, and China. "It was scorbutic dysentery that was so fatal at Rangoon in 1824; and it is always apt to occur in transports, where ventilation and food are bad."† The principal remedies here, of course, must be constitutional, to improve the diseased blood by diet, &c.; but, till these have time to take effect, cannot we do much by saving, by injections, the intestine from irritation and ulceration? and why should astringents, tannin in its various forms, acids, &c., have less beneficial effect applied locally to the bleeding vessels and ulcers of the colon than we see them to have on the spongy bleeding gums and ulceration of the mouth? In this disease, all you require is, to delay the destructive process in the bowel till diet and other measures take effect; and is not the colon only second to the stomach‡ in introducing potash, or whatever medicine our theory may prefer, into the blood?§ Three pints of water are

\* Inspector of hospitals there.

† Dr Johnson's Rev., p. 379, Oct. 1846.

‡ Query, is the colon second to the stomach in its *absorbent* function? It is so in its *digestive*, no doubt; but the colon must have this also in some degree, or how was Dr Watson's patient nourished for a month and a-half?

§ See Medical Register, May 1848, p. 256.

absorbed from it in an hour; and Dr Watson, p. 644, cured a case of stricture of the œsophagus, by feeding his patient entirely by the rectum, with beef-tea and egg, three times a-day, for five weeks and a-half. Let it be remembered, too, that, in stopping the irritation, diarrhoea, and ulceration of the bowel, you add greatly to the present strength of your patient, which, in scurvy, is so essential for its cure.

I have thus considered almost all the complications of dysentery. But, as there may be those who will still deny that abscess of the liver is caused by inflammation of the veins, or absorption of acrid matters from the colon, I shall mention all the other modes of accounting for it, and show that, adopt what theory you will, injections must be absolutely essential.

Dr Parkes thinks that abscess of the liver is caused by contamination of the blood with those substances which are usually excreted by the solitary glands of the colon. I do not think he can have many followers in this theory; but, even if he has, they will scarcely deny that it would be as well to foment with hot water (poultice) these glands, and restore their secretion as quickly as possible.

Again, some say, the secretions of the liver being very much altered by sympathy with the irritation of the bowel, that this diseased bile causes inflammation of its substance and abscess. Dr James Johnson remarks,\* "That the liver suffers more or less seriously in every case of dysentery, is proved by the well-attested fact, that the bile is invariably altered in its qualities, or, in many cases, all but suspended. It becomes green, thick, viscid, stringy, with numerous crystalline particles suspended in it." The evident cure for this is, to clear out, without delay, all the irritating matter from the intestine, and inject opium, which will then stop this sympathetic irritation of the liver.

Annesley and others say that dysentery is often caused by the diseased bile irritating the bowel as it passes; and many think that dysentery, united with typhus, is caused from the elimination, by the intestine, of the typhous poison, which is mixed with the blood. In both these cases, our duty clearly is, to prevent the accumulation and delay in the colon of this acrid bile and poison, by diluting and washing them out frequently by injections.

*Conclusion.*—The writer in the *British and Foreign Review*† says, "inflammation of the solitary glands has been considered by some late writers as the first morbid condition in dysentery; and the existence of these bodies only in the large intestines, has been considered to be the cause of the limitation of the disease to these parts. On the other

\* Rev. p. 377. Oct. 1846.

† Oct. 1847, p. 336.

hand, several eminent anatomists describe the process as always one of rapid, and, in the first instance, superficial inflammation, leading inevitably and speedily to mortification, and unattended with any special disease of the solitary glands. Rokitsansky states, that, even in the first or slightest variety, the mucous membrane is red, and may be removed in the form of pulp from beneath the epithelium (cuticle of the mucous membrane); while, in the after stages, and, in the severer varieties, the mucous membrane is transformed into a gelatinous and easily separable substance, or is, in the worst form, in a state of complete sphacelus, black, friable, and offensive."

These solitary glands, having a lower vitality, slough and suppurate much sooner than the surrounding mucous membrane; and because the colour of this slough on their apex (a yellowish white) is permanent, while the congestion and vascularity of the mucous membrane, in which they are imbedded, disappears, as it usually does after death, except the membrane be partially disorganised, they found these solitary glands yellow, and all the membrane surrounding them apparently healthy, and framed all kind of theories to account for it. Some likened them to apthæ, some to small-pox. Some even went so far as to assert, "Dysentery\* is an epidemic scabies of the intestine, as is evident from the dissection of the dead;" and ascribed it to some specific poison of the blood, as in small-pox, &c; but seeing that dysentery was produced frequently by malaria, they were obliged to divide malaria into two kinds,—one producing dysentery, and the other fever. The writer in the *British and Foreign Review*† calls them "analogous, but not identical causes;" and supposed that, when ague and dysentery are united, both these poisons circulate in the blood at the same time. Others not satisfied with thus inventing a new poison for their theory, say‡ (p. 339), "The intestinal lesion is the effect of a chemical action taking place between the glands and the morbid matter circulating in the blood." In modern theories, when in a difficulty, how often is chemical action, no one knows what, brought into the rescue. But fortunately, Dr Baly, though himself holding formerly the above opinion, has brought at last the matter to the test of anatomy, and, by extensive *post mortem* examinations, shown not only the mistake, but the *reason* of the mistake, and, as usually happens, the facts observed are found true enough. The theories only attached to them are absurd.

The following is, for the most part, an abstract from his Gulstonian Lectures, delivered at the College of Physicians, London, in 1847.

The colon consists of its mucous membrane and epithelium (cuticle). Below this is the vascular layer or tissue, which sup-

\* *British and Foreign Review*, p. 342. October 1847.

† *P.* 349. *do.*

‡ *British and Foreign Review* for *do.*

plies it with blood ; and certain bodies are imbedded in the mucous membrane called solitary glands, which are also nourished from the vascular layer beneath. In dysentery the earliest changes occur in the vascular tissue, which is beneath the mucous membrane and solitary glands. This tissue being attacked with inflammation, the blood stagnates in the capillaries, which supply the mucous membrane and glands above it. Thus deprived of nourishment, these extra vascular parts perish, and are thrown off as sloughs. If the inflammation be moderate, the solitary glands chiefly suffer, because they are the most destitute of vessels. If, however, it be more severe, the glands only suffer in common with the other extra vascular parts, viz. the mucous membrane, and are thrown off in common with it. Thus are formed the larger ulcers in the severer forms. The apparent special affection of the solitary glands, and the general sloughing of the whole mucous membrane, are only degrees of the same action ; and the solutions of continuity in the mucous membrane, of whatever size and shape they may be, are all derived from the same process, viz. one of mortification and sloughing. Thus, in a milder case, the glands alone, from their more defective circulation, slough ; while the mucous membrane around them only arrived at the congestive stage, fades, and appears perfectly healthy after death, thus bringing prominently into view the solitary glands, from the yellow-white colour of their sloughing, making them look very like the pustules of small-pox or scabies. But if the inflammation be severe, the mucous membrane, glands and all, lose at the same time their vitality, and are thrown off together in one mass. "The glands lose their vitality, and, together with the portion of the surrounding tissue, in which the circulation has ceased, are thrown off as sloughs." (Baly, p. 11.)

If, then, it be now proved, that there is nothing specific in the cause of dysentery, and considering that it is constantly produced by the most common and varied causes,—a chill, bad water, food, diseased liver, &c.,—the wonder is that it was ever thought to be specific. How is it, that except occasionally in the dysentery from scurvy, the disease is entirely confined to the colon ? The writer in the *British and Foreign Review* (p. 335, Oct. 1847) says, "It is almost *unnecessary* to say, that all agree in considering dysentery to be a disease chiefly, and in some forms entirely, of the large intestine ; and in the first instance, of the mucous coat." Again (p. 139), Dr Wilson, Inspector of Hospitals in China,—“but the numerous autopsies which have taken place, show that the primary and permanent source of the intestinal symptoms has been in the colon.” Parkes (*Johnson's Review*, Oct. 1846) says, “In all the cases I have examined, the small intestine was free from disease.” How is it, I ask, that while the causes are general, affecting the whole

intestine, small and large, that the large alone is seriously diseased, and that the inflammation and sloughing, as Annesley and others show, suddenly stop at the valve of the colon, and that in the colon itself, its two extremities, the cœcum and sigmoid flexure, are the parts which most severely suffer? Malaria, checked perspiration, bad food, water, acrid secretions of the liver, &c., equally affect the whole intestinal canal. Why then does the colon *alone* suffer? My answer to this question is the following, and it has never been answered before.

In animals, but particularly the horse, where the stomach is very small, the food, and even water, rapidly passes from the stomach through the small intestines, to be digested in the colon. In the stomach and small intestines it is reduced merely to a pulpy liquid form, but when it arrives at the cœcum, the valve closes on it, and being shut up between the sphincter at the anus, and this valve, it is delayed for days, sometimes weeks, till its fluid and nutritious portions are absorbed, and the remainder at last slowly ejected as dry and hardened excrement, no longer containing any matter capable of absorption. The cœcum in the horse is very large, and is called its "water stomach," from the rapidity with which water taken into the stomach passes into it through the small intestines, to be there detained and absorbed. Any one comparing this with the colon of a man, must plainly see that it is formed by nature to accomplish the same function. There is the valve to shut it up from behind, and the sphincter and sigmoid flexure to close its other orifice; and its contents are slowly passed upwards against gravity, and detained by gravity in its horizontal portions, before they can even approach the descending portion and the rectum. They are even prevented from pressing too hastily on the rectum, and thus exciting their too early evacuation, by the beautifully contrived twistings and serpentine bendings of the sigmoid flexure.\* This sigmoid flexure is copiously supplied with sympathetic (organic) nerves, which close its passage during the 12 or 24 hours, during which we feel no inclination to stool; but in the morning, or at other times, principally guided by habit, the sigmoid passage relaxes, the fæces pass into the rectum, and we feel the desire to evacuate. This has been well shown by O'Beirne; and that the rectum habitually remains empty, may be satisfactorily proved by any one so inclined, by passing up a finger.

It is well known, that if the habitual time be neglected, the desire for evacuation passes off, and the fæces may be detained for weeks, and be themselves even absorbed, as is often proved by the fetid breath and perspiration of those who are habitually cos-

\* See a very good plate of them, fig. 40, Churchill's Midwifery.



tive. I have seen cases where there could be no doubt that the skin excreted fæcal matters thus absorbed.\*

When a mucous membrane is inflamed, its secretions, even when quite clear, seem always more or less acrid. Witness the tears from the eyes excoriating a child's cheek in ophthalmia, and the copious use of tallow grease in England, to protect the nostrils and upper lip when excoriated in catarrh. If then the mucous membrane of the whole line of intestine be inflamed from any cause, such as checked perspiration, &c., where would its secretion and the bad food, water, bile, &c. produce the greatest irritation? Plainly in that part where they are most detained, viz. in the colon. These all pass rapidly along the small intestines, but being shut up at the two orifices of the colon, there irritate by their detention, its already inflamed surface. This increased inflammation produces still more acrid secretions, and these mixed with the fæces found there, and gases, sulphuretted hydrogen, &c., produce a compound, which, if not removed, can scarcely surprise us if it produces rapidly very violent inflammation, quickly leading to ulceration. Now suppose (fig. 7) a tube of this kind, closed at the two extremities *a* and *b*, and partially filled with fluid or semifluid matter. This, if moved about in it, would, by the action of gravity, necessarily accumulate more or less equally at the two extremities of the bend *a d* or *b c*. Now read what Parkes says (p. 14), and compare it with the above,—“Two important anatomical varieties of dysentery, are those in which ulcerations are found, chiefly in the cœcum, and those in which they are principally confined to the rectum. The varieties are also generally the result of *local irritants*, and produce peculiar symptoms. Thus, when the cœcum is the part affected, there is great pain on pressure over the cœcum, and a very perceptible fulness in that region, arising either from arrested fæces or secretions, or inflammation and œdema of the coats of the cœcum. If the inflammation runs high, ulceration and destruction of the ilio colic valve ensues.” [The cork rots from the burning nature of the contents of the bottle.] “When the rectum is principally affected, there is intense tenesmus; the stools are nearly pure dark blood, and if the case be neglected, some portion of the mucous membrane speedily sloughs and protrudes through the anus.” [The cork at the other end rots, if the acrid fluid run over principally to that side.]

Now, Parkes was a man who devoted himself entirely to the *anatomy* of dysentery, and had quite different theories from me,

\* I have before shown, in Part I., that fluids and even gas, in large quantity, are detained by the swollen sigmoid flexure. Dr Macgregor (p. 105) says, “in colitis after death, fluid liquid fæces are found in great quantity through the whole length of the colon, and distending the cœcum to an enormous extent, which seems to be the great reservoir of fæcal matter.”

and yet does not nature force him to say the same thing as I do above, and do not all others agree with him in these facts? Now suppose this experiment;\* take any part of the living intestine, small or large, of an animal, place it in a syphon shape as *a, d, c, b* (fig. 1), with the bend uppermost, and obstruct with a gentle pressure of two fingers each end, *a* and *b*, so as not to destroy the circulation. Suppose this portion of intestine partly filled with a strong solution of sulphate of zinc, and move it about in the intestine, till part of the liquid remain at *a*, part at *b*, the two ends of the tube, would not these two ends be the first to inflame, ulcerate, and rot? Would not the rest of the intestine more or less escape, except where in moving the fluid the solution of zinc was brought in contact with the more prominent tops, *a, b* (fig. 8), of the folds, wrinkles, of the mucous membrane? and would not the parts *e, f*, between them, and therefore concealed and partly covered by these folds, suffer less?

Dr Raleigh had great experience of dysentery in the General Hospital, Calcutta, and says (p. 29), "The mucous coat preserves its integrity in the spaces or sulci between the elevated masses, whilst *over* the elevated portions it is extensively abraded by sloughing ulceration." Dr Baly says (p. 338), "These gangrenous portions were generally situated on the transverse folds or plicæ." The writer in the British and Foreign Review says (p. 344), "This (effused) lymph has generally occupied the *summits* of the plicæ."

Sydenham† is celebrated for his close observation of nature, and he saw dysentery in its very worst form, viz. united with typhus, when 2000 died of it in London yearly,‡ and read how positively he speaks:—

"The reason is, that the corroding secretions easily flow off in the small intestines, and therefore cannot cause *there* excoriation and corrosion, as happens in the colon, where the morbid secretions being entirely stagnant, corrode without limit the mucous membrane. Therefore I have found (he says), as I should have supposed, that abstergent clysters and warm water is the best" (my doctrine, you see, entirely). Bampfield too says (p. 228), "experience has *fully* persuaded me, that the morbid secretions excoriate and excite ulceration in the villous coat of the intestines."

Sydenham had only the short clyster pipe, which is almost useless in the case of obstruction of the sigmoid flexure; but if he had had the long tube, would he not, holding these opinions, have instantly used it, and placed his principal reliance on it in his

\* I do not say that this experiment is practicable in a living animal, I only use it to illustrate my meaning.

† De Curatione Dysenteriae, 1818, p. 11.

‡ Heberden on the Increase of Disease in London. 1801.

treatment? And can I be wrong in concluding, that dysentery in its primary nature, as seen in the small intestines, and parts of the colon protected from the secretions, is a mild and harmless disease, and that, therefore, if we remove quickly these acrid secretions, we shall disarm dysentery of its terrors? If it be complicated with marsh fever, typhus, or scurvy, we must treat these too, and in doing this have we not gained half our battle, by calming the constitutional irritation of their most dangerous symptom, reacting so fearfully on the original fever; and do we not prevent the great danger of the absorption of poison, by the known absorbing powers of the colon, and thereby fresh contamination of the blood and the putrid symptoms resulting?

The substance, then, of the whole argument is this. The long tube changes a huge internal abscess into an external, and enables us to wash out and cleanse from it its putrid contents. It also enables us to foment and soothe, by *local* applications, the sloughing and ulceration these contents have caused on its surface. If any one can deny the utility of these means, I confess I cannot understand him. The consideration, too, that this abscess is in a vital organ, quickly prone to ulceration and sloughing, and moreover that its *function* is absorption, and therefore that it will rapidly absorb its own putrid contents, adds doubly to the force of the argument.

On those who may in future try this system I would strongly urge, not to mix the old treatment with it, and dose their patients with calomel and purgatives which are directly opposed to it, and not to be led away by false theories of hydropathy\* and absorption of water, which are utterly without the shadow of probability, and to give the injections without limit in quantity but the patient's endurance. You cannot injure a piece of dead intestine by distending it with water, much less the living, which quickly contracts powerfully on its contents; and do not cease repeating the injection till you are satisfied that the colon is evacuated and cleansed. I would refer to Dr Hathaway's case (Register, June 1848) as a specimen of the decided and unyielding determination thoroughly to evacuate the colon, as the first essential to be accomplished, which I myself practise; as for his being accused for not bleeding this patient, what good could bleeding do (he did not require it), or any other treatment? Do you bleed your patients for your own amusement?

*Note.*—In writing the above, I do not refer to Dr Withcombe's cases in the Register, October 1848. His object was to

\* Med. Register, Calcutta, p. 635. Dr W. says, "Nevertheless, I am certain that it has more than a mechanical effect, and that hydropathy may explain some of the wonders of the long tube." The idea was first taken from Dr Baddeley's remarks on his own case in the number for March 1848.



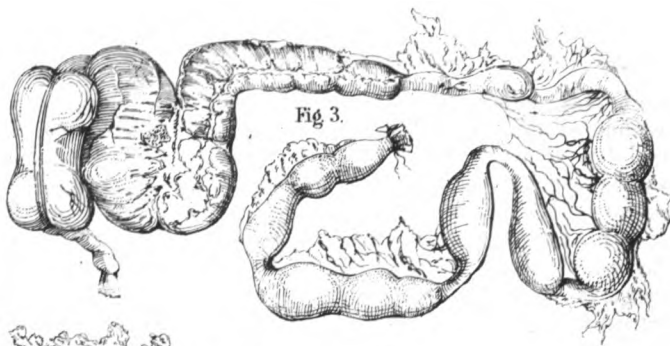


Fig. 3.

Fig. 4.

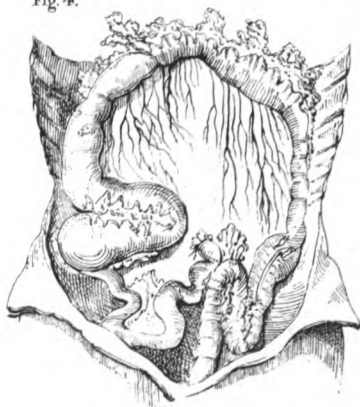


Fig. 2.

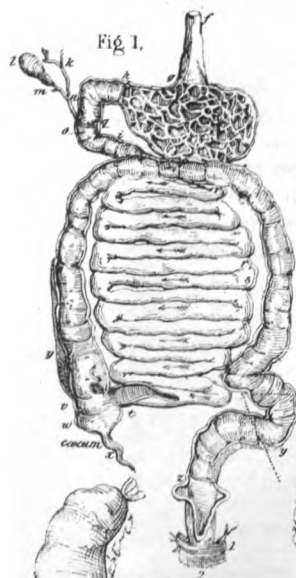


Fig. 1.

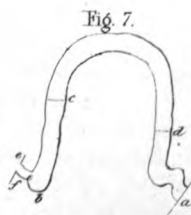


Fig. 7.

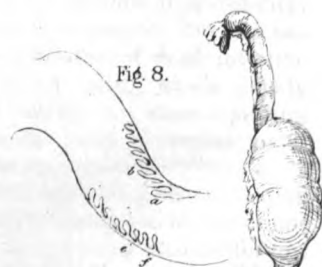


Fig. 8.

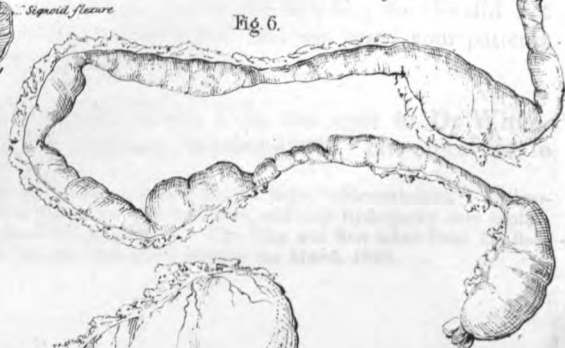


Fig. 6.

Fig. 5.



show, that, *after* the failure of the common system, which was first fully tried, what "wonders," as he calls it, the long tube could produce, even in his patient's *extremity*.

July 14. He is much worse this morning, passing no natural stool whatever, and detached pieces of mucous membrane have come away. He has been delirious during the night, and is now very low.

Nothing could well be more hopeless than these symptoms. But mark the result, when the injections were commenced.

July 17. Just had an enema with the long tube, and passed an amazing quantity of most filthy matter with the water when it returned. On the 18th, he was convalescent; on the 20th, well with "nil nisi injectis."

Dr W. remarks, "I have selected this case from several which have been successfully treated in the same manner, because it is more parallel to contrast with the fatal one in which the patient was of the same age, same temperament, and constitution, and with symptoms very similar. The long tube was employed in the *latter*, and not in the *former*, who, although he had everything else done for him, died, while the man Walts, by mechanically cleansing the bowel, is alive and well. The most extraordinary accumulations of filthy matter have been removed from the intestines by the means alluded to."

The author hopes to be able to publish these cases referred to, and a large number of others he has collected, in a future number of this Journal.

### Explanation of Plate.

References to Figure 1. Diagram from Elliotson's Physiology. Healthy Structure.

*f*, The œsophagus; the upper part having been cut away to shorten the figure.

*g*, The Cardia.

*h*, The pylorus; the space between *g* and *h* being the cavity of the stomach.

*i*, The duodenum.

*k*, The hepatic duct

*l*, The gall-bladder.

*m*, The cystic duct.

*n*, The *ductus communis choledochus*, formed of the two.

*o*, The opening of the *ductus choledochus* into the duodenum.

*p*, The pancreatic duct.

*q*, Its opening into the duodenum, which here is distinct from that of the *ductus choledochus*.

*r*, The *jejunum*.

*s*, The *ileum*.

- t*, The termination of the ileum in the cœcum.
- u*, The superior fold of the valve of the colon.
- v*, The inferior fold of the same valve.
- w*, The cœcum.
- x*, The vermiform process.
- y y*, The colon.
- z*, The rectum.

1. Part of the *Levatores ani*.

2. Anus.

Fig. 2. From Annesley. Plate xxiv. Vol. ii. p. 101. Diseased Structure. Wylley's Case.

Fig. 3. From Annesley. Plate xxvi. Vol. ii. Case cliv. p. 98. Vallery's Case.

Fig. 4. Annesley. Plate xxx. Cavanagh's Case. Vol. i. p. 492.

Fig. 5.. From same. Connelly's Case. Vol. i. p. 261.

Fig. 6. From same. Dilatation and contraction of the colon.

Fig. 7. *a, b*, the colon; *a*, anus; *b*, cœcum; *f*, valve of cœcum; *c* and *d*, surface of fluids; *e*, cut end of small intestine.

ART. IV.—*Observations on Diseased States of the Placenta, as influencing the process of Parturition.* By JOHN BREMNER, Surgeon.

It is well known that the placenta, notwithstanding the ephemeral nature of its existence, is liable to become the seat of disease of various descriptions, to such extent as more or less to destroy its organization, and consequently to unfit it for the proper accomplishment of its peculiar function. This holds not less true, although it may not be possible to determine with accuracy, in every instance, whether the morbid state has originated in that connecting medium betwixt mother and child, the texture of the uterus, or the fœtus itself.

In scrutinizing the different modern works respecting intelligence on this subject, it will be found that the information to be derived is both scanty and undigested in its nature,—having reference chiefly to the kind which tends to render difficult its separation from the uterine *parietes*, without in any degree assisting in the exposition of their true character. The only exception to the statement here given, so far as I am aware, is to be met with in the Edinburgh Medical and Surgical Journal for April 1836, being the contents of a paper read by Dr Simpson, the present Professor of Midwifery in that University, before the Royal Medical Society, 20th November 1835, in which is con-

tained, in a concise form, a much more ample and correct history of the diseases of this organ than had previously been published. The morbid conditions treated of by Dr Simpson in that paper, are those of placental congestion and inflammation,—affections which he deems probable may arise from a variety of causes, and believes to prove destructive beyond most others to foetal life.

But whilst these states of the placental mass have thus been ably investigated and detailed by this gentleman, with the assistance derived from the labours of others, but slight attention has been directed to the influence which they seem capable of exerting over the natural progress of labour,—or, in other words, to the degree of resistance which they oppose to regular and efficient uterine action, in the absence of all other well-marked causes of like tendency.

Although I had, since the commencement of my professional career, from time to time met with cases of the description about to be related, the space of five years has not elapsed since that of a *primipara* occurred, where, in the absence of every particular source of interruption perceptible, the accouchement was of the most obstinate and tedious kind. This forms the subject of Case 5 of the present selection, where an ample detail of its history, together with an enumeration of the reasons which co-operated in leading me to the conclusion already intimated is to be found. I have never heard or read of any similar opinions on the subject.

In a small treatise recently published, chiefly upon venesection and opium in obstetric practice,\* a few cases of this description have been inserted for the purpose of illustrating other topics; and in a note appended to p. 92 of that work, a more ample history is promised to be rendered in a separate form.

The sketch, as now presented, contains a list of the leading phenomena relative to this peculiar disease, which I have attempted to collect and illustrate in as brief space as possible, since my removal from the sphere of practice where they were witnessed. It is chiefly compiled from reports of the cases taken at the bedside during the progress of the several accouchements treated of,—a plan which the anomalous nature of most of these first suggested when I had little else to engage my attention, without any view to publication; and also, where doubt existed, from inquiries made at a few of the patients of very recent date.

Before proceeding farther, it remains to be mentioned, that the illustrations refer to those conditions only in which there is an exemption from all unnatural adhesions betwixt the uterus and placenta, but where upon expulsion the latter is found to have its organization more or less destroyed; and in many instances emit-

\* Hints on Obstetric Practice, with Illustrations, by John Bremner, Surgeon, Newtyle. Part i. Edinburgh, 1829.



ting an effluvium of an offensive nature,—reserving till towards the concluding part of the discussion what observations or suggestions may arise out of their history or from any other source.

As the difficulties which had to be contended with in the greater number of the labours yielded to the prompt exhibition of venesection and opium, agreeably to the systematic plan laid down in the work referred to, it has here been found necessary considerably to abridge the treatment by means of these remedies, referring those who may feel interested to the tract mentioned in the previous page, for a detailed account of the properties they possess, and the laws which ought to regulate their administration.

*Case 1.*—On the afternoon of 19th December 1837, when passing I was requested to visit Mrs C., who was in labour for the first time, and had been attended by a midwife for the space of twenty-four hours. Upon farther inquiry, it was ascertained that she had been five years married, and that her age, which she seemed unwilling to disclose, was upwards of 40.

Upon examination, the *os uteri* was found dilated to about the compass of a farthing, and so attenuated as to be with some difficulty distinguished from the membranous sac. The patient was a good deal inclined to shivering, with rapid and feeblish pulse; pains from the commencement having been extremely trifling and irregular; and she being then unfit for venesection, an enema, containing about four grains opium in solution, was ordered. I then left her, and returned at 7 o'clock, when but slight alteration was found either on the dilatation or state of the pains; and also no inclination to sleep. Opiate enema desired to be repeated, after which I went homeward for the night; having instructed the midwife to attend to the bowels, and also to re-administer the opium as circumstances might demand.

Upon my arrival the following day about noon, the account received was, that she had enjoyed several hours of tranquillity after the second dose of the opium, but about 4 A. M. of 20th the pains had arisen again to such height without effecting any favourable impression, as to render necessary the exhibition of a cathartic enema, followed by a third repetition of the opium, which had the effect of restoring quiet and some desire for sleep, till a short time prior to my visit, with a total exemption from headach, &c. No change upon the *os uteri*. Pulse feeling fuller and slightly excited; from six to eight ounces of blood was taken from the arm, and a fourth repetition of the opium during the afternoon, in consequence of which I learned, upon my return about 2 P. M. of 21st, that she had passed a very comfortable night. Upon examination, dilatation of *os uteri* had advanced to the circumference of a halfpenny, but was again at a stand. Directed opium to be repeated in a larger dose, the effect of which

only lasted till about 7 o'clock, by which period very perceptible enlargement of the orifice had taken place.

About this time, believing that the strength of the pains, which was now much augmented, would soon exhaust my patient without the smallest prospect of a favourable termination to the delivery, no alternative remained but to repeat the opium once more in a dose considerably increased. This had the effect of throwing her into a profound sleep for several hours, without inducing headach or any unpleasant affection. After this the progress of the labour became regular through the night; the intervals betwixt the pains being passed in sleep. At 8 A. M. of 22d, the head being fully descended into the pelvis, the pains also having declined greatly in number and efficiency, as well as the strength of my patient, the forceps was applied, and in about an hour and a half the delivery of a daughter was effected, with the face directed towards the *os pubis*; a circumstance I had not before detected.

In the record of this very lingering and notable case, which was taken down during the progress of the labour, no mention is made of particular delicacy on the part of the child; whilst upon the removal of the placenta, which required the introduction of the hand, appearances of disease had begun to manifest themselves, but of which I possess not now a perfect recollection. The health of the mother had been extremely delicate for some time prior to confinement, and she was much reduced in point of appearance as well as strength. After this event she recovered rapidly, and, with the child, did well.

*Case 2.*—I was called, on the evening of the 23d March 1836, to visit Mrs M'K., about five miles distant, who had already given birth to several children, with comparatively easy deliveries; and on the present occasion had been attended by her former midwife for twenty-four hours, who had bled her and administered a cathartic and two opiate enemata, on account of fruitless fatiguing pains, with which the patient was much distressed. Finding, however, that the labour made no progress, and that she could not, upon examination, reach the *os uteri*, she, with the approbation of the husband, deemed it proper that farther assistance should be called.

About 10 o'clock, when I reached the house, examination into the state of the labour soon convinced me of the truth of the report given by the midwife; and had it not been for the reliance placed upon the early performance of venesection, and the succour so afforded by the exhibition of the opium, the case appeared such as from which I would with cheerfulness have pleaded leave of absence, seeing that little other prospect appeared, should the patient's strength begin to give way, than to terminate the process,

if possible, by turning, or reduce it to the third class. It was resolved, however, to make some farther effort before having recourse to any of these expedients.

Having in possession only an undetermined quantity of muriate of morphia, a single grain, as nearly as could be calculated, was instantly administered as enema, with the view to subdue the spasmodic action which still from time to time continued to annoy the patient. In doing this it partly succeeded, but without effecting any favourable change on the *os uteri*, which could only with difficulty be reached, or procuring rest.

Matters being in this situation at the expiry of an hour, a repetition of the dose was ventured upon, by means of which a total cessation of pains, as also a state of sleep, was soon after induced, such as I had not before witnessed in obstetric practice. The patient lay extended on her back, with her mouth widely opened, and snorting for the space of several hours like a person labouring under the most profound apoplectic stupor.

About 4 A.M. of 24th she began to awake, and for a short time the pains continued moderate, recurring at intervals of several minutes. By and by they became a scene of violent unintermitting conflict, compelling the patient to scream aloud. Upon examination, the change effected upon the parts was so great, that the birth of a son was safely completed about 5. Although in all respects lively, still he was considerably under the average size, and several livid patches were discernible upon his body. He soon after birth became uneasy and mournful, was seized with convulsions, and died at the expiry of forty-eight hours.

The placenta upon removal was in a corrupted state, giving out a very offensive odour. The mother had a good recovery, and, during two successive accouchements, got through in her former easy manner,—the children in both cases lively and thriving.

In course of time she became pregnant once more, and was seized with labour pains on 26th September 1841, being attended by a young practitioner who had a short time previous settled in the district. Finding that, at the expiry of twenty-four hours, the process had not in good earnest commenced, I was requested to visit her on the evening of the 27th. Upon reaching the house the account received was that she had enjoyed a very tranquil day, being only occasionally disturbed by slight unavailing uterine efforts, for the improvement of which few remedies had been employed.

Upon examination, the *os uteri* was found to a certain extent dilated, and free from thickening or tenderness of its edges. Under these circumstances, opium, in the form of enema, was administered as before, when she slept soundly during the night. With the approach of morning labour commenced in earnest, and

was completed by the natural efforts a little before 3 P.M. of the 28th,—the patient requiring, for the proper regulation of the pains and support of her strength, at least two repetitions of the opium ; and likewise a draught or two of infusion of spurred rye.

During the day it was discovered that several discharges of meconium had taken place, leading to the belief that the life of the child was extinct. The case, however, proved to be otherwise ; but the infant was extremely delicate, and scarcely survived the period of the former,—the placenta being found to present the exact resemblance of the other.

Upon revisiting this patient, on the second or third day after delivery, it was easy to perceive, by means of the shrunk, pale appearance of the features, in connection with the accelerated state of the pulse and heat of surface, that disease somewhere existed, and was making rapid inroads on the system ; and which, from the similarity the symptoms bore to certain former cases met with, taking also into consideration the state of the placenta, could be traced to no other organ so likely as the uterus, although nothing satisfactory could be elicited after careful examination. The symptoms of irritative fever continued to advance and increase, and, after a short time, a severe pulmonic affection commenced, in consequence of which respiration was performed with much difficulty, attended with distressing cough and expectoration, which speedily reduced the patient's strength, and terminated her existence in little more than three weeks from the period of her confinement.

*Sectio Cadaveris.*—Upon making the section of the abdomen, and dividing the parietes of the uterus, its mucous membrane, more especially the seat of the placental attachment, presented a roughish black appearance, seemingly as if in a great measure abraded, but without any points of ulceration on its surface, or thickening of substance.

With reference to the state of the pulse observed in Mrs M'K.'s case, I beg to say that the circumstance is one of very considerable importance, and derives no small confirmation from a quotation given by the late Dr Hamilton, from Dr Clarke of London, in his "Select Cases of Midwifery," published in 1795, p. 35, viz. "That where the pulse continues quick above twenty-four hours after delivery, all is not well." I did not meet with Dr Hamilton's work, however, till two years after the occurrence of this.

Although not in the order of time, it is considered most proper here to relate the history of the following cases.

*Case 3.*—Mrs T., aged about 40, who had already given birth to several children, expected to be confined in May 1845. I had been called in on account of the lingering nature of her first

labour, and, although not present at the delivery, was afterwards informed by the midwife that the child was still-born, and the placenta a mass of disease. I likewise attended her in two subsequent confinements, and, in the one preceding that about to be related, was obliged to deliver by embryulcia, on account of the arm presenting along with the head. In all these cases, the condition of the placenta was the same; the lives of the children seemed uniformly extinct before delivery took place.

Being thus aware of the peculiar temperament of this individual, I had agreed to attend her from the commencement of the labour in question. Calling to inquire after her on the 22d instant, and finding the pulse moderately excited and wiry, I took from her arm eight or ten ounces of blood, which, upon cooling, was, to a certain extent, sisy, with a rather loose coagulum. She stated the period of her accouchement as being quite at hand, and I accordingly received a call to visit her on the succeeding day, viz. the 23d, in the afternoon.

I found, upon my arrival, that, although the pains had been rather frequent and strong, the *os uteri* could not as yet be reached; on which account, five grains of opium in solution were exhibited by the rectum, which procured sleep during the greater part of the night. I left her early in the morning, and, having received no intimation, returned on the evening of the 24th, when I learned that she had remained comparatively quiet till four P. M., a recurrence of pains having then taken place without effecting any change on the *os uteri*. Nine P. M., opiate enema repeated with an effect similar to the former.

I remained till four A. M. of the 25th, when no change on the *os uteri* could be detected, although the discharge from the orifice, which had been present from the commencement, left no room for doubt, that a disunion of its edges had occurred. Upon my return, about two P. M., I found she had enjoyed ease till towards mid-day. The *os uteri* could now be reached, dilated to the size of a farthing; edges thick, but flabby, and void of tenderness on pressure.

From this period, the labour advanced progressively, but slowly, requiring one or two additional exhibitions of the opium to regulate the pains and sustain the strength, till five A. M. of the 26th, when the birth of a daughter naturally took place, in which the powers of life were extremely languid; but, by recourse to active measures for upwards of an hour, became ultimately restored, and, together with the mother, did well. The state of the placenta, upon removal, the same as usual.

She once more became pregnant, and was seized with the precursory symptoms of labour about midnight of December 13th, 1847. I saw her at one P. M. of the 14th, and found her much

in the same situation as on the preceding occasion, save that there was an entire absence of arterial excitement, and she considered she had undergone a greater reduction in her strength than before.

In relating the particulars of this case, suffice it to say, that a plan, very similar to that of the foregoing, was adopted, so that, by six o'clock P. M., the head had descended into the pelvis, without wholly filling up the hollow of the sacrum, but could not, by any means, be made to advance farther. Being provided only with the short forceps, they were attempted to be fixed; but the task was for some time considered as fruitless. Having, however, several miles to send for those of a larger description, a few additional attempts were made, and, happily, with better success; so that, by eight, the delivery of a son was completed, which, it was hoped, would have stood an equally fair chance of life as the former. The result, however, proved otherwise,—the vital spark having wholly fled,—although, previous to delivery, the mother seemed repeatedly conscious of its movements. Upon the removal of the placenta, it was found not to have attained the same degree of disorganization as the others, but seemed slightly indurated, and considerably reduced in size.

*Case 4.*—Mrs H., whom I had attended during six previous confinements, the greater number of which were completed in from two to three hours, and all of the most natural kind, was seized with labour pains early on the morning of the 9th April 1848.

For some considerable length of time, she had felt much more uncomfortable than usual, and easily fatigued; and latterly experienced great difficulty in moving about. But having had an abortion since the birth of her former child, these affections were attributed chiefly to the debilitating effects consequent upon it. On reaching the house, at half-past ten o'clock, she was in bed, complaining a good deal on pains located wholly over the *pubes*, without, upon examination, having exerted the smallest influence on the *os uteri*, for the improvement of which the opiate enema was prescribed, and of necessity repeated different times during the day (the bowels also having been particularly attended to), but with less effect than could *à priori* have been anticipated,—the pains never diverging from their original situation, by which means their salutary operation in advancing the labour was much curtailed and interrupted.

Notwithstanding these obstructions, the process was terminated about nine P. M. by the natural efforts, the child manifesting but extremely faint symptoms of life, and ultimately restored only with the greatest difficulty. The placenta and also the funis and membranes were found in a very morbid state. Although there was throughout the day an entire absence of headach, and but

slight annoyance from sickness, it was found impossible in this case to suspend wholly the action of the uterus, or induce a state of sleep by means of the opium.

Not having it in my power to revisit this patient till after the expiry of a good many weeks, I was then informed that, over all the extremities of the child, and parts remotely situated from the heart, an almost complete separation of the epidermis took place. I never met with a similar instance where life was preserved; but it is one which I conceive tends forcibly to illustrate the true nature of the affection, and the languid state of the circulation which had for some time been going on. Both mother and daughter soon became healthy and strong.

*Case 5.*—I was requested on the evening of the 7th October 1844, to visit Mrs C., a young woman in labour for the first time. Upon reaching the house, about two miles distant, I found her of rather slender habit, but presenting a very lively appearance, and was informed, upon inquiry, that, during pregnancy, she had enjoyed almost uninterrupted health. She stated having been annoyed with occasional acute pains during the afternoon, without exerting any particular effect on the *os uteri*, which could not be approached without the use of immoderate force. Under the circumstances enumerated, and the state of the bowels having been ascertained and rectified, the pulse also being found regular and natural, opium was administered, as in the preceding cases, which procured ease and sleep for a considerable part of twenty-four hours; and, upon a recurrence of similar symptoms in the course of the following afternoon, but without any sensible impression on the *os uteri*, a repetition of the former treatment was adopted, and with the same effects. By the approach of the morning of 9th inst., the *os uteri* could be felt with ease, slightly opened, and presenting a very natural dilatable appearance.

The pains, however, during the course of the day became occasionally very troublesome, without any sensible difference on the dilatation, requiring to be allayed by means of the opium in doses of increased strength; and, notwithstanding which, to such an aggravated height did they proceed about ten P. M., yet without material advancement in the size of the orifice, that I felt somewhat at a loss to conjecture how the case might terminate, fearing lest rupture of the uterus or some such casualty should be the result.

Notwithstanding the apparently desperate nature of the case, no mode of practice with which I was acquainted could be imagined so deserving of confidence as perseverance in that which had been adopted from the commencement. Opium was therefore administered in the manner described till a total suspension of uterine action occurred, and a state of sleep produced for at

least four or five hours, little inferior in degree to that of Mrs M'K., Case 2. With the advance of the morning of the 10th inst., in proportion as the patient began to emerge from her profound slumber, pains began to recur, and speedily completed the birth of a still-born daughter, for whose recovery no means of resuscitation employed proved in the least effectual.

This seemed at first view rather surprising, seeing that it had not been exposed to any noxious influence of which I was aware. But when upon the removal of the secundines, similar circumstances were distinctly visible, as in the examples already detailed, and after a short time spent in reflection, and a contrast made respecting the same lingering and formidable character of all the accouchements put together,—a key seemed not only to be obtained whereby the cause of death could receive a satisfactory explanation, but also as tending, with nearly an equal degree of certainty, to prove that the unusual delay and difficulties with which they were more or less accompanied, could not be referred so clearly to any other source.

The ideas so forcibly suggested to my mind upon the termination of this case have been amply verified by those of more recent date.

Upon a further review of the most usual phenomena presented by the cases in question, it appears that the delay and obstructions standing in the way of moderately expeditious and safe parturition proceed chiefly from a more than ordinary torpid or debilitated state of the body of the uterus, and are not, in the majority of them, at all, or but very slightly, connected with those conditions of the orifice and cervix which frequently exert such powerful effect in retarding, as well as augmenting the dangers, attending upon the process. In order to arrive at clear and impartial conclusions respecting the condition of the organ here described, it has only to be borne in mind that, by reason of its intimate connexion for any length of time with the placenta, where the healthy state of the latter has by any means been destroyed, the uterus must of necessity become more or less contaminated with the morbid influence, and be thereby rendered unfit for the due performance of its natural function.

In many of the patients the muscular energies seem, as it were, more or less paralyzed; and in proportion to the strength of constitution and the length of time the poison has been operating, in perhaps the same ratio will the tone of the general system be found to have suffered. In support of this, several notable examples could be adduced, where the strength was completely undermined before parturition took place. Such aggravated results are without doubt much accelerated by the death of the fœtus; which,



notwithstanding, is frequently carried in this lifeless state a considerable period.

In the case of twins, sometimes the death of one will be productive of very distressing effects when the other is born alive.

With certain exceptions, as in that of Mrs T., No. 3, and without including those instances where the infant's death is occasioned by maltreated and protracted accouchements from other causes,—which, consequently, do not come within the scope of the present observations,—the commencement of the evil will be found to originate in the placenta.\*

In by far the greater number of the cases met with, the sanguiferous system was found to be seldom or but very slightly excited in comparison of others; and in those in which the phlogistic state existed, the fibrinous incrustation, together with the crassamentum, formed a very imperfect coagulum upon cooling,—circumstances uniformly denoting either a very delicate or a vitiated and asthenic state of the system.

Upon a more minute and particular survey of the cases, the reader must perceive, that whilst the progress of the accouchements were all more or less lingering and tedious, a very remarkable diversity prevailed amongst the greater part of them, with regard to the symptoms by which they were characterised, as well as the condition of the fœtuses when ushered into the world. In none of them was there found such an exemption from derangement of health as No. 5, who, although naturally endowed with a constitution of considerable delicacy, affirmed that she had been free from every complaint for at least several months previous to confinement. The odour of the placenta, nevertheless, had become very offensive, and livid patches appeared upon the child soon after birth. I have only met with one such other instance, the history of which is related in the work referred to at the commencement of this discussion.

As a general rule, all such individuals present, more or less, a sallow, drooping appearance of the countenance, and lowness of spirits, along with general muscular relaxation; and are the almost constant victims of grinding, fluctuating pains, but which can frequently at least, be traced to some part of the uterine region as a centre; as also unpleasant nervous sensations which they can scarcely describe.

\* In tendering this statement, it has not been overlooked that the death of the fœtus itself may be, and undoubtedly is, sometimes the primary operating cause—as in hydrocephalus and other diseases. But although it would prove highly interesting to be able to distinguish in any peculiarly doubtful case betwixt the one and the other, still it seems upon the whole to be a matter of rather trivial importance, as the practical bearings must eventually be the same.

In every suspected case of difficult labour, the chief aim of the intelligent accoucheur ought to be, to ascertain, if possible, the existence or otherwise of the child, in the event of craniotomy being found indispensable.

From among many other cases not included in the preceding list, I would make a brief selection of one or two, in whose history the circumstances were perhaps even still more remarkable.

The first is that of a female whom I had attended during six or seven previous natural confinements. This patient began to feel so uncomfortable betwixt the seventh and eighth months of gestation, that for several weeks previous, upon attempting to walk through her chamber, and even sometimes in the act of standing up, she was in great danger of falling, in consequence of a violent spasmodic state of the parts situated about the pelvis and flexor muscles of the thighs, together with a sense of weight which seemed impossible to be carried about, and compelling her to remain almost motionless until assistance was obtained. Being then unacquainted with the peculiarities of the affection, of all the ideas entertained respecting such unusual symptoms, that of a plurality of children was fixed upon as the most likely; the critical period being anticipated, and spoken of with interest. It having at length arrived, the birth of a daughter, in every respect strong and lively, was effected in about double the ordinary length of her former accouchements. The placenta, upon removal, being found diseased to an extent equal almost to any I had ever witnessed, afforded an explanation of the obscure parts of the case.

The second is that of a *primipara* of delicate constitution, who, during pregnancy, had suffered severely from bad health, and for some time previous to confinement, could only with difficulty walk about, or remain long out of bed. I visited her about four o'clock A.M., and was enabled, after her strength was greatly exhausted, to deliver her by the forceps, about noon of the same day. The child, a boy, was alive, but much below the average size, and, notwithstanding his extreme weakness and puny figure, survived for the space of nine days. The secundines presented the same morbid appearances as described, but she had never experienced interruption to the discharge of the urine, or pain on going to stool.

It was therefore supposed that with the accouchement the difficulties would have terminated. Upon the succeeding day, however, I received a hurried message to visit her on account of total suppression of urine. Relief having been obtained, and certain directions given as to the patient's future management, it was hoped that no additional visits might be required. On the succeeding forenoon, the same urgent request was repeated, but still with equal obscurity as to the cause of the incident. Instead of improvement, matters became worse, considerable tumefaction and tenderness occurring about the *pudenda*, so as to render it in some degree difficult to pass the catheter. No benefit was derived from

the application of leeches, fomentations, and other local means, so that, for twenty successive days, I regularly rode the distance of five miles, and went through the operation (no one in the vicinity being qualified to undertake it), without being able so much as to conjecture to what inexplicable combination of circumstances such a phenomenon could be ascribed. On the twenty-first, however, the mystery was suddenly and unexpectedly solved, in consequence of an abscess having burst, and a large discharge of foetid matter taken place by the rectum; whereupon, the interruption to the flow of the urine immediately disappeared. I was never able to arrive at any conclusion as to whether the disease of the placenta was the cause of the abscess, or *vice versa*.

With regard to the recurrence of the affection, it will be observed, that, with the exception of Cases 2 and 3, it was not again met with; thus affording the surest evidence that the placenta was the source from whence it originated. In Mrs M.K.'s second confinement, it seems beyond dispute that the uterus was affected from its connection with this organ in a highly morbid state,—she having in the meantime given birth to two children, with labours of the simplest kind. What the peculiar feelings of this patient were during the periods referred to I never could properly ascertain; but that they bore a very striking resemblance to each other during the respective pregnancies seems to be verified by the circumstance, that she repeatedly informed her husband, prior to her last confinement, that the issue would be exactly similar to the former; at the same time presaging with much earnestness the event which a little time was destined so fully to accomplish.

As to Case 3, a very different conclusion only can be formed; the strongest proofs being afforded, from the number of repetitions witnessed, that the uterus was the only source of the malady, although the patient all the while presented a very healthy appearance in the intervals, and continues to do so to the present day. Mrs T. is only one specimen of several others which could be adduced; and there is but small reason left for doubt that hers is that state of the uterus in the incipient degree which regularly throws off the embryo betwixt the fourth and sixth or seventh months of gestation, and almost uniformly, sooner or later, terminates the life of the victim by means of disease concentrated in that organ.

The confinements of Mrs T. exhibit a very striking contrast to each other. In the first, it will be observed that, although it was extremely lingering, and the secundines much diseased, still a degree of phlogistic action prevailed in the system, the strength of which, when compared with the torpid state of the uterus, was but slightly reduced; and consequently, when the tone of the latter

was brought into equilibrium therewith, the labour advanced without interruption, and was completed by the unaided efforts.

In the second, the state of matters appear to have been nearly reversed. The more energetic condition of the organ can be readily accounted for by the smaller degree of disorganization of the placental mass; so that the accouchement, though somewhat tedious, would have been concluded naturally within the same limits, but for the greater prostration of the patient's strength than in the former instance. This at least appears the most rational view of the question which can be taken.

The fate of the children seems involved in much greater obscurity.

With respect to the influence of age on this complaint, a review of the cases witnessed in the course of practice bears me out in affirming, that no particular period of life is exempt from its attacks in preference to another, but that it pervades every stage and description of constitution almost in an equal ratio.

Did I consider myself at liberty, from the limited number of examples placed within the sphere of my own observation, to decide as to the status of the individuals who form its most frequent objects, I would say they are the opposite of those, who enjoy robust health and constitutional vigour; or who, amongst that class, have given birth to several children, or had their systems, to a certain extent, reduced by floodings and other debilitating causes. I have also met with it pretty often in females far advanced in life before giving birth to children. It by no means holds good, however, that the very weakly and delicate portion of the sex are peculiarly exposed to it more than others.

It has been suggested, in objection, that the delay alluded to can bear reference only to primary labours, without being at all affected by the cause specified. To perceive at once the fallacy of such an opinion, it is believed that the comparison of Cases 2, 3, and 4, in connection with their former history, will furnish abundant evidence.

The difficulty of procuring sleep and a change of the pains with regard to locality in Case 4, was peculiarly observable, and tended in no small degree to augment the delay and trouble attendant upon it, lest the patient's strength should have become prematurely exhausted.\*

In turning from the mothers to the consideration of their offspring, a variety of peculiarities will also be found to present themselves to notice. 1st, That, with the exception perhaps of

\* I may here remark that, whilst in Case 1, the *Secale Cornutum* might have been very profitably employed, so in this chloroform would have proved an excellent adjuvant; but I was unprovided with either of these at the moment,—a circumstance I did not afterwards regret as to No. 1, having obtained ample scope for testing the powers of opium in rousing the dormant energies of the uterus.

Case 1, and the one alluded to at p. 67, they were all born in a very weakly or asphyxiated state, from which it was found impossible to restore some of their number, viz. Mrs T.'s last child, and Case 5, which seems to prove that positive assurance respecting the state of the child cannot be calculated upon *à priori* in any instance of the disease. 2d, That certain others did not average much above half the ordinary weight at birth; nevertheless, were then comparatively lively, and survived from the second to the ninth day. 3d, That the degree of disease in the secundines and the life or death of the children seem to bear small relative proportion to one another. 4th, That, though the passage of meconium previous to delivery has, by writers on the subject, been generally considered an unfailing proof of the death of the child, yet this does not appear to be borne out by the evidence of the cases above related, having only been discovered in Mrs M.K.'s second labour, where the child was born alive; the utmost degree of confidence attachable to it, tending to prove that, if it does not indicate the certain death of the fœtus, it denotes a state of great general weakness, in consequence of which the sphincter muscles have become relaxed. 5th, The value of stethoscopic observation in all suspected or doubtful cases, before having recourse to destructive measures of delivery. 6th, That, as life, under circumstances like the present, can only be preserved by the prompt use of energetic measures, it seems a duty binding upon every accoucheur to have these constantly in readiness, and never to relax his efforts, so long as the most distant prospect of success remains.

*Causes.*—Concerning the causes of this affection, when the uterus and its appendages are in a sound state, it is to be feared that the present amount of our knowledge is very limited. Whatever tends to interrupt or disturb the regular connection of the placenta and course of the circulation with any of its respective media, must undoubtedly prove inimical to the healthy condition and operations of the same. Under this head may be comprehended blows inflicted immediately over or in the vicinity of the organs alluded to; sudden frights and leapings; the lifting and carrying of heavy burdens; to which may be added, a too copious or diminished supply of blood to the parts.

After much inquiry, however, into the circumstances of all the cases witnessed for many years, I feel every assurance in stating that they are, with the exceptions already quoted, as yet involved in much obscurity; none of the patients being capable of affording the smallest information on the subject, whilst nothing could be referred to disturbance of the circulation.

*Diagnosis.*—In attempting a correct diagnostic chart for the detection of this very obscure affection, I am aware that the

difficulties to be encountered are by no means small or few in number.

With the view to forward this object as far as possible, the following list of symptoms, a good many of which have been already enumerated, will be more or less discernible in the majority of cases, according to the duration and progress of the disease upon inspection. A more lank, pale, and drooping appearance of the countenance than natural, along with flaccidity of the mammæ, and diminution of adipose matter throughout the muscular interstices and cellular membrane. Frequent attacks of pains are experienced, confined chiefly to the back and uterine region; but these are often not so distressing in degree as a constant general irksome state of the system which prevails, rendering the patient's situation extremely uncomfortable by day as well as night, and which anodynes can but very partially remove. Such being the state of the system, that of the circulation must also of necessity participate. Instead, therefore, of the incompressible wiry pulse, so easily distinguishable in the greater number of individuals from the seventh month to the full period of gestation, the reverse is met with; or, if the former prevails, it may in almost every case be viewed as having existed previous to the occurrence of the disease, which invariably mitigates greatly the phlogistic influence. This is particularly evident, when, upon the approach of labour, and the dilatation of the *os uteri* commences, its edges are occasionally found more attenuated than in most exempted cases; or, if otherwise, the enlargement will be observed to proceed chiefly from serous infiltration, and, consequently, to be but slightly affected with pain upon pressure with the finger.\*

I have been not a little surprised to witness this exempted condition of the organ in patients, wherein it had formerly been extremely difficult to divest it of the opposite tendency; nay, even where the state of the pulse at the same time called for the moderate abstraction of blood from the arm, thus manifesting decisively the asthenic character.

It has long been considered as a law in the economy of the gravid uterus, that it becomes much reduced in volume in the direction of the *scrobiculus cordis*, in consequence of a relaxed state of the pelvic ligaments, and perhaps other causes, a short time previous to confinement, and is one of the surest harbingers of its being at no great distance. In certain of the cases related, but especially that of Mrs C., No. 5, the distension of the

\* Of all the cases treated of, this attenuated state was most remarkable in Nos. 1 and 5, especially the former. Until the accouchement of the latter took place, I was always wont to refer the extreme torpidity of the uterus in the former to the advanced age of the patient,—a circumstance, however, which I could never by any means reconcile with the condition of the orifice described.

organ was as great as ever, a circumstance which, for a short period, made me hesitate whether the labour could be accounted genuine. I will not avouch it to be universal, but feel inclined to believe it a very general occurrence, depending upon functional derangement of the uterus by means of its morbid contents.

The principal affections, with which it is most liable to be confounded, are those of an inflamed state of the *os* and *cervix uteri*, and nerves issuing from the lumbar vertebræ or sacrum. In the first, however, the pains are generally more of the lancinating kind than met with here, and, unless the vascular system be also highly excited, give most annoyance during the night season, advancing often when unrestrained, with the strength and regularity of those of genuine labour pains, and well entitled to the appellation "spurious." The general appearance and condition of the patient will also afford considerable additional proof. A careful examination of the spinal column by means of the hand will serve to distinguish it in every instance from the last. Stethoscopic investigation will render essential aid in ascertaining the vitality or otherwise of the child. It must, however, be very evident, that a combination of the former will more or less be frequently met with, as also of the latter and other neglected or imperfectly treated diseases, which intelligence and attention on the part of the practitioner cannot fail readily to recognise. The symptoms here recorded are not warranted to apply where other diseases of the uterus or syphilis exists.

It is only, however, upon the birth of the child that the real nature of the case comes to be understood. Immediately on the head and shoulders being extricated, it either remains wholly motionless, and instead of the continued fit of crying which for a short time occurs in the sound state, a faint murmur or two only is heard; the features are shrunk, the action of the heart is remarkably feeble; the body in general presenting an almost lifeless appearance. The exceptions met with will not be numerous.

Upon the removal and inspection of the *funis* and membranes, the following appearances will be discovered. Instead of the pearly white colour of the former, a much more sombre dirty aspect is presented. The blood-vessels have exchanged for the palish blue colour that of a yellowish or sea-green; which is best perceived when they become ramified upon the placenta, in consequence of the thinness of the covering. Upon subjecting the mass itself to examination, occasionally the odour it emits is sufficient evidence of disease. This, however, is not uniform, and oft-times nothing disagreeable is to be detected by the olfactory organs,—in which case there is seldom much alteration in the texture or size of the organ, which I have generally imagined to become heavier, and more easily separated into shreds, in proportion

to the degree of decomposition it had undergone. Sometimes, as in the case of Mrs T., it appears to be diminished in size and weight, as well as more compact than usual.

I have been in the habit for several years of attaching greater importance to the state of the blood-vessels and coverings than to that of the placental mass, and been able, if I mistake not, in many instances, to trace by means of the appearance they presented, the influence of the poison over both the labour and fœtus, better than could have been done by an appeal to it; whereas, had I not chanced to become acquainted with the delay in question, I should have imagined the whole, as formerly, to be matters of course.

*Treatment.*—Seeing that all prospect of effecting the cure of this affection is hopeless, the most useful measures to be pursued will be found to be those of a prophylactic and palliative nature.

The first class will comprehend, the avoiding, as far as possible, all exciting causes, with attention to the state of the circulation during the seventh and eighth months of pregnancy; whilst the use of the lancet, if indicated, the exhibition of opiates, attention to the bowels, and gentle cordials, will be found more or less serviceable, where it may be suspected to have occurred, till labour arrives. In cases where a fixed pain proved very distressing in some part of the abdomen, I have witnessed the best effects from an opiate plaster.

Should the view of the question presented to the consideration of the profession in the preceding pages be found substantiated by future observation, it is presumed that through such a medium several circumstances possessing a certain degree of interest may perhaps receive a more satisfactory explanation than hitherto they have done,—amongst which the following may be ranked as specimens. 1st, The vulgar opinion that labour is rendered more difficult when the death of the fœtus takes place *in utero*, seems with greater justice referable to that of the placenta than the child. 2d, It will be observed that in a good many, I think I may safely add, the greater number, of the cases quoted, the action of the uterus, laying aside its inefficient nature, was trivial and indolent in the extreme, as in Mrs C., No. 1, &c.; and it is a well-established fact, that for many years this is the description which has been regarded by the best obstetric writers, as the most proper for the administration of the *Secale Cornutum*, a medicine which, though latterly fallen somewhat into disuse, has frequently been productive of the happiest results.

It has, however, unfortunately happened, that whilst its virtues, as a remedy in stirring up a more energetic state of the uterus, have been fully admitted and appreciated, it has, on the other hand, been found no less true that, upon the termination of the



labours, many of the children have proved to be still-born, which their mothers declared, and other symptoms concurred in rendering probable, to have been but a short time before possessed of life. As injurious effects have been uniformly found to result from the use of this substance only when consumed in too great proportion in admixture with the healthy grain as an article of food, the unfavourable result, it is conceived, may therefore be very readily explained, upon the supposition that the cases so terminating are generally of the character here specified; and this opinion seems to receive no small degree of confirmation from the fact, that the incident has been observed to be by no means of unfrequent occurrence, when only a very moderate quantity of the medicine has been administered, whereas exemptions have perhaps been met with nearly as numerous after the exhibition of large and repeated doses.

I beg to say, that it is no part of my intention to maintain, that all the examples met with in practice will be equally lingering and tedious with those I have thus detailed. I have been informed, on most respectable authority, that the number in which the accouchements are gone through with ease and celerity is by no means small:

It must, however, be recollected, that before such a statement can be received as valid, it will be necessary to furnish a complete history of every case in all its bearings. With the exception perhaps of No. 1, I have no hesitation in affirming that, but for the disease in question, the accouchements of all the others would have been concluded within twelve or fifteen hours from the commencement of my attendance upon them. The former labour of Case 4 scarcely occupied more than three hours, and is the same recorded No. 15 of the work recently published; whilst the succeeding one of No. 5 only required five.

I cannot call to remembrance so much as one single instance in practice, where the delivery was unattended with a certain degree of delay and difficulty, attributable chiefly, if not solely, to this cause; for it so happens that the excited and unyielding states of the *os* and *cervix uteri*, as also of the external parts, are in a great measure absent when this is present, for the reason formerly stated. A rigid state of these parts in consequence of advanced life can scarcely be expected to be so easily counteracted.

It is only by means of an acquaintance with all these circumstances, carefully and candidly weighed in the balance, that any approach to correct inferences on the subject can ever be drawn, as well as opportunities afforded, of establishing the respective claims of all the different remedies and rules of practice recommended for the numerous and formidable list of grievances which it is the inevitable lot of the parturient female to be subjected to,

when that critical moment does arrive, more especially within the bounds of civilized life.

Impressed as I have for several years been with a conviction of the truth of these opinions, as well as their importance in a professional point of view, and open at all times to demonstrative evidence from every well substantiated source,—in drawing this inquiry to a conclusion for the present, I am not without the hope that it will meet the attention of those who, uniting intelligence and a regard only for knowledge derived from inductive experimental research, are placed in much more suitable spheres for observation than I have ever enjoyed.

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ART. V.—*On the Fatal Cases of Inhalation of Chloroform.*

By JOHN SNOW, M. D. (Read at the Westminster Medical Society, March 31, 1849.)

SOON after the introduction of the inhalation of ether, two or three cases occurred in which it was rumoured that fatal effects had followed the practice; and in one case, that of Mrs Parkinson at Grantham, a coroner's jury returned a verdict to that effect, without much inquiry; the cause of death being taken for granted by the coroner in his charge. However, as the physiological effects of ether became generally known to the profession, it became evident that these deaths, which occurred two or three days after severe operations, could not be attributed to the inhalation. I know only one case where death was occasioned by the inhalation of ether.\* This occurred in France, and the process was continued for ten minutes, without intermission, although alarming symptoms existed a considerable part of that time, and the result was probably as much due to defective admission of air as to the influence of the vapour.

At the time at which the inhalation of ether was introduced, two solutions of chloroform in spirit were in occasional use as medicines,—one of them bearing the name of chloric ether, and the other being called terchloride of carbon. The former of these preparations was used occasionally for inhalation, in St Bartholomew's Hospital and elsewhere, soon after the employment of sulphuric ether was introduced; but, besides being expensive, it was uncertain in its effects,—partly from its variable strength, but chiefly because the chloroform evaporated in largest quantity at first, leaving the spirituous solution weaker and weaker as the process continued. The composition of this so-called chloric ether was not generally known to the profession; but in the latter

\* See Gazette Medicale, Mars 4, 1848.

part of the year 1847, Mr Waldie of the Apothecaries' Hall of Liverpool, being in Edinburgh, made known its nature, and recommended the chloroform, to which it owed its virtues, to Dr Simpson, who was at that time in search of new anæsthetics. Dr Simpson tried the chloroform in its separate state, as recommended by Mr Waldie, found it to answer, and introduced it into general employment, as is well known; and in a short time it almost superseded the use of ether throughout Europe, and became employed also, to a great extent, in America.

Chloroform is much more powerful than ether, and this is one reason why it is, in some respects, more convenient. Its greater potency depends, as I showed on a former occasion,\* on its being more sparingly soluble in the blood than ether. The quantity of chloroform required to induce insensibility is less than one-tenth as much by measure, as in the case of ether. Viewed in this manner, it is more than ten times as strong; but to ascertain their comparative physiological power, when inhaled in a similar manner, their volatility requires to be taken into account. In order to perceive the relative strength of these two medicines, we may suppose that the air which a patient breathes is saturated at 60°,—the ordinary temperature of a dwelling room,—with one or other of the vapours, and see how much air he would have to breathe in either case, in order to be narcotized to the third degree,—the extent of insensibility usually required in a surgical operation. Thirty-six minims is about the average quantity of chloroform required to produce this degree of narcotism in the adult, and this would saturate 257 cubic inches of air at 60°, making it expand to nearly 300 cubic inches, which would be breathed in 12 ordinary respirations of 25 cubic inches each. The quantity of ether usually required to produce the same amount of insensibility in the adult, is about  $7\frac{1}{2}$  fluid drachms; this would saturate 440 cubic inches of air at 60°, and increase its volume to rather more than 800 cubic inches, which would require 32 ordinary respirations to breathe it. We see, therefore, that 12 inspirations of air charged with vapour of chloroform are equal to 32 similar inspirations of air charged with vapour of ether, at the same temperature; and that, consequently, chloroform is nearly three times as strong as ether. In actual practice the difference in strength is generally greater than this, for ether abstracts much more caloric than chloroform during its evaporation, thereby reducing the temperature of the air passing over it, and the sponge or whatever contains it, and limiting its own evaporation, in a greater degree. It follows, therefore, that the fact of accidents not occurring under the use of ether, could be no guarantee that they would not happen during the employment of chloroform.

\* See Med. Gaz., March, 1848.

Having, on a previous occasion, described the ordinary effects of chloroform, I shall now proceed at once to the consideration of the circumstances under which it is capable of causing death. When an animal, after it has become completely insensible, is allowed to continue breathing air charged with the vapour, the respiration shortly ceases; but if the air do not contain more than about five per cent. of the vapour, the heart continues to pulsate for some time after the breathing has ceased, and the circulation is finally arrested for want of the respiration, as in all other cases when death takes place by apnoea. I have heard the pulsations of the heart, by means of the stethoscope, several times, for one or two minutes after the breathing has ceased, in cats and rabbits under the influence of chloroform. During this interval, life is easily recalled by means of artificial respiration; indeed, more than once, moving the animal, or pressing on its chest, whilst using the stethoscope, has apparently been the means of resuscitating it. This persistence of the heart's action, as I have elsewhere shown,\* does not arise from any incapacity of chloroform to paralyze it, but from the circumstance that the sensibility of that part of the nervous system on which the motions of respiration depend, is abolished by a somewhat smaller quantity of the narcotic than is requisite to suspend the action of the heart. When, however, an animal is made to breathe air containing a greater quantity of the vapour, ten per cent. or upwards, death takes place rapidly in from half a minute to about two minutes, and the respiration and circulation cease about the same time. The reason of this is, that there is sufficient vapour in the lungs, at the moment when the breathing stops, to paralyze the action of the heart as soon as it is absorbed, and added to that already contained in the blood. Under these circumstances, it is evident that artificial respiration can be of no avail; and this is the manner in which, there is every reason to believe, the greater number of the fatal cases of inhalation of chloroform have occurred.

In order to give as clear an idea as possible of the cause of the accidents from chloroform that have happened in different parts of the world, I must direct attention to some considerations of the quantity of it, in the blood and in the lungs, under different circumstances. I have stated that the average quantity of chloroform required to be inhaled, by an adult, to induce the usual amount of insensibility required in an operation, is 36 minims; but about half of this is expired again without being absorbed, so that 18 minims is about the quantity existing in the blood. Indeed, about a year ago, I related to this Society† the result of some experiments, undertaken to ascertain the exact proportion in the blood, in the different degrees of narcotism, by which it was shown

\* *Med. Gaz.*, Vol. xlii. p. 415.

† *Ib.* xli., p. 850.

that about 12 minims is the quantity in the second degree, or the stage when the mind wanders and voluntary motion is unsteady; about 18 minims in the third degree of narcotism; about 24 minims in the fourth degree,—the stage of complete insensibility, with relaxation; a little more than 30 minims, the quantity that would be required to suspend the respiration; and about 36 or 37 minims to arrest the action of the heart, supposing the vapour to be equally diffused through the whole of the blood. Now, let us ascertain what proportion of this latter quantity of chloroform may be present in the lungs at once, provided the air breathed by the patient be saturated at 60°. The quantity of air usually present in the lungs is about 250 cubic inches; this would contain 30 minims before it entered the lungs, but, as absorption and inhalation go on together, it is probable that, during ordinary respiration, only from half to two-thirds of this quantity would be present in the lungs at once, or from 15 to 20 minims; and, if the chloroform were removed from the patient's mouth, part of this would be expired again, and not more than from 10 to 15 minims would be absorbed into the blood after the discontinuance of the inhalation. But, supposing 12 minims were thus absorbed, they might cause a fatal result; for if the patient were narcotised to the third degree, and about 18 minims had been absorbed, when the inhalation was discontinued, the additional 12 minims would make half a drachm, which might suspend respiration; or, if the patient were already in the fourth degree, and twenty-four minims had been absorbed, twelve minims more would complete the quantity which I have estimated would be sufficient to arrest the action of the heart. These numbers are only approximations to the exact quantities; but the subject itself is not one of speculation, for that the effects of chloroform continue to increase during about twenty seconds after the inhalation is left off, owing to the further absorption of the vapour present in the lungs, may be observed daily in practice, even when the chloroform is largely diluted with air, and, of course, the greater the quantity of vapour in the air, the more formidable must be this increase of the narcotism. It will be recollected that I called the attention of the society to this increase or accumulation of the effects of chloroform, early in January last year,\* before any fatal case had happened from its use, and recommended that the vapour should, by means of some suitable apparatus, be so diluted, that its effects should be gradually induced in not less time than about two minutes.

Sometimes a patient begins all at once to breathe deeply during the inhalation; and, under these circumstances, if the vapour be not largely diluted, it will be inspired with dangerous rapidity.

\* See *Med. Gaz.* Vol. xli. p. 75, and *Lancet*, Feb. 12, 1848.

The first incision by the surgeon's knife, when the patient is unconscious, but not totally insensible, sometimes has the effect of causing him to draw a deep inspiration, and to hold his breath at the end of it, retaining the air in his lungs: now, an inspiration of this kind might, on a moderate computation, introduce 100 cubic inches of air; and, if this were charged with vapour of chloroform, by passing over a handkerchief or sponge, it might contain ten or twelve minims; if the air or the handkerchief were warm, it might, indeed, contain much more; but this quantity, added to that already in the circulation, might cause a fatal accident.

It must be sufficiently evident, from these considerations, that, unless some means were used for regulating the strength of the vapour, fatal accidents would be liable to occur from the employment of chloroform. Unfortunately, Dr Simpson, to whom we are indebted for its introduction, recommended it to be used on a handkerchief, and even held it out as one of the advantages of the new anæsthetic, that it did not require any apparatus. This advice, coming from so high a quarter, could not fail to meet with numerous followers; and to this circumstance, many of the accidents that have occurred must, in my opinion, be partly attributed. It is not asserted, by the advocates of the handkerchief, that it affords any accurate means of regulating the strength of the vapour. This they seem to think unnecessary. They appear to consider that all that is required is, that the patient should inhale the chloroform till he is made insensible, and then leave off;—that the practice is not attended with danger; and that the deaths which have occurred, either were not caused by the chloroform, or depended on some idiosyncrasy, or the presence of organic disease. In order, however, to show the different effects of the vapour, according to the quantity of air with which it is diluted, I will make two experiments; in the first of which, a bird will die in about half a minute, although having sufficient air for purposes of respiration; and, in the second, another bird will become gradually insensible, and will as gradually recover, after its removal from the vapour.

Twelve grains of chloroform were poured into a glass jar holding 100 cubic inches, and covered with a plate of glass; the jar was moved about till the chloroform was converted into vapour, and equally diffused amongst the air, when a brown linnet was introduced. It made an attempt to escape, then appeared in distress; shortly afterwards it fell down, and it was taken out dead half a minute after its introduction. The same quantity of chloroform was then put, in a similar way, into a jar holding 600 cubic inches, and another linnet introduced. After a short time, it began to stagger as it walked, and soon afterwards was unable to keep on its legs; in about two minutes it appeared insensible;

but was allowed to remain a minute longer, when it was taken out and laid on the table. It shortly began to move, and in two or three minutes had quite recovered.

The jar employed in the first experiment holds enough air to support the respiration of a small bird, for ten or fifteen minutes, without distress. The chloroform introduced formed about nine cubic inches of vapour, displacing as much air, and leaving about 91 cubic inches; but the same quantity of vapour of ether would have acted much more mildly, gradually causing insensibility; and the same quantity of some vapours,—that of wood spirit, for instance,—would have scarcely had an appreciable effect on the bird in several minutes. Vapour of the strength employed in these two experiments does not act so rapidly on larger animals, but, by increasing its strength, it acts as quickly, and there is the same difference in its effects, with a similar difference in the proportion of vapour and air.

The first fatal case of inhalation of chloroform was that of Hannah Greener, which occurred near Newcastle, on the 28th of January last year. The patient was a girl 15 years of age, who required to have her toe nail removed. About a teaspoonful of chloroform was put on a cloth, which was applied to the mouth and nostrils. In about half a minute the patient's arm was rigid, and she appeared insensible. The operation was at this time commenced; she gave a kick, as if not quite insensible; at this time the lips became suddenly blanched, and she spluttered at the mouth, as if in epilepsy. The breathing continued for upwards of half a minute after the cloth was removed from the face, but the patient was quite dead in two minutes from the first commencement of the operation.\* The spasm that occurred in this case at the moment of death, resembles what is often met with when animals are suddenly killed by vapour of chloroform not well diluted with air.† The most prominent appearance, found on inspection of the body, was a highly-congested state of the lungs. Sir John Fife attributed the death to this, but it ought rather to be attributed to the narcotic effects of an over-dose of chloroform on the brain and nerves. Both chloroform and ether, when in too strong a state, cause a temporary congestion of the lungs, as is evidenced by a momentary smallness of the pulse; but it may fairly be doubted whether this would cause death, and especially in so rapid a manner; whilst, on the other hand, the undoubted narcotic effects of the vapour explain it fully. There is no evidence to determine whether the action of the heart continued in this case after the respiration had ceased or not. There was dark fluid blood in its right cavities, and a very little in the left.

\* See Dr Meggison's Letter, *Med. Gaz.*, Vol. xli. p. 254.]

† For a full account of the case of Hannah Greener, see the *Edinburgh Medical and Surgical Journal*, Vol. lxi. p. 498. April 1848.

Dr Simpson attributed the death in this instance to suffocation, by a small quantity of brandy given to the patient whilst she was dying, and which she swallowed, though with difficulty; but I believe that few medical men agree in this opinion. Some persons,—and there are medical men of eminence amongst them,—whilst they admit that the chloroform was the cause of death in this and similar cases, suppose that it acts mechanically, by excluding the air; but they do not distinguish between a vapour and a gas. Chloroform, when not admitted to mix with air, remains liquid until it is raised to the temperature of 140° Fah., unless the pressure of the atmosphere be partially removed from it, by means of the air-pump. I have already alluded to the real cause of the accident, viz. that the air breathed by the patient contained too much vapour,—probably 10 or 15 per cent., when it ought to contain only about 5. There are others who think that the handkerchief applied too closely has caused death, by obstructing the respiration; but where death has happened so suddenly, this explanation is quite untenable. When ether was first introduced, I believe that some disagreeable symptoms were occasionally caused by obstructed respiration, from breathing for several minutes through a very narrow tube; and I have alluded to one case, in which this process being continued for ten minutes without ceasing, probably contributed to a fatal result. But the accidents from chloroform are of a different nature, and it must be borne in mind, that with this medicine air is required to dilute its vapour considerably over and above what would be merely required for the purposes of respiration.

The next accident from chloroform happened to Mrs Simmons, of Cincinnati, U. S., on Feb. 23, 1848. The limits of a paper will not allow of the cases being read in detail, consequently I can allude only to their chief features. The patient in America was 35 years of age, and in pretty good health. She required some stumps of teeth removed, and inhaled from a glass globe containing a large sponge saturated with chloroform. I believe that this is the only fatal case in which any kind of apparatus was used, and the dentists who employed it were not medical men, and their only endeavour appears to have been to make the patient insensible as quickly as possible. When the patient had drawn from twelve to fifteen inspirations, she appeared insensible, and the roots of teeth were extracted; she turned pale during the inhalation; groaned as the teeth were removed; and as the last root came out, her head turned to one side, her arms became rigid, and the body was drawn backwards. Mrs Pearson, a friend who accompanied the patient, at this moment placed her finger on the pulse and found it feeble, and that it shortly ceased to beat; the respiration ceased, she says, about the same time. She and



another female friend considered that the patient died at this time, about two minutes from the commencement of the inhalation,—the dentists thought that she lingered a little longer. Artificial respiration was performed after a time, and galvanism was also applied, which caused contraction of the voluntary muscles, but had no effect on the heart. In a medical inquiry which took place respecting this case, it was estimated that one-fourth part by measure of what the patient inhaled, might be vapour of chloroform; but this is evidently an over-calculation, for there could not be this quantity, unless the interior of the glass globe were maintained at a temperature of  $80^{\circ}$ ; however, half the amount supposed might have caused the result.

At the *post mortem* examination, the blood-vessels of the head were moderately distended; some fluid blood mixed with air flowed from the sinuses of the *dura mater*,—this air had no doubt entered the blood-vessels during the artificial respiration. The lungs were considerably, but not intensely, congested; the heart was flaccid, and all its cavities were quite empty, but its lining membrane was stained with blood,—it had been emptied after death, either in opening the head, or else by the artificial respiration. Dr Sibson, in commenting on this case, last year, in the *Medical Gazette*, stated that he had seen the heart emptied after death, during experiments with artificial respiration in the human subject. The blood was very fluid in this case.

The next case occurred at Hyderabad in Hindostan, in the person of a young woman, who had the distal phalanx of one of her fingers removed. A drachm of chloroform was put on a handkerchief and inhaled. The patient coughed a little, and then gave a few convulsive movements. The operation was then performed, but scarcely a drop of blood flowed, and no signs of life remained. The practitioner is inclined to think that death was almost instantaneous; for, after the convulsive movements, the patient was not observed to breathe or stir. There was no examination after death.

The next fatal case happened at Boulogne, in May last. The patient was a female, aged 30 years, in good health, but she had previously been in a state of anæmia. In the first account of the case, it was stated that only fifteen to twenty drops of chloroform had been put on the handkerchief; but a judicial examination of the bottle from which it had been taken, showed that from a drachm and a quarter to two drachms had been used,—five to eight grammes. “Scarcely had the patient taken several inspirations, when she tried to remove the handkerchief, and cried, ‘I choke.’ Immediately the face became pale; the countenance changed; the breathing embarrassed; and she foamed at the mouth; at the same instant (and certainly less than a minute

after the beginning of the inhalation), the handkerchief moistened with chloroform was removed," and the operation was performed by M. Gorré, his colleagues, in the meantime, trying in vain to remedy the state of the patient. Artificial respiration was kept up for a long time, and with such force as to cause permanent dilatation of the air-cells.

At the inspection of the body, the lungs were found visibly engorged in the lower lobes. The heart was quite empty, as in the American case, in which artificial respiration had been employed. The vessels of the head were not engorged, but air was found in the venous sinuses. It was met with also in the pulmonary veins (where no doubt it first entered by minute ruptures of the air cells and vascular tissue), in the heart, in the right carotid artery, and in the veins generally.

The members of the Academy of Medicine of Paris, attach great importance to the air in the veins; most of them attributing the death to it; some supposing that it was due to an unknown action of chloroform on the blood, and others thinking that it was spontaneously and suddenly developed. It is alleged that the air in the veins could not have been introduced by the artificial respiration, as the left ventricle had already ceased to act. But so soon as an opening was made into the pulmonary veins, the air would be urged forward, in the course of the circulation, by the forcible inflation. Moreover, the arteries possess the power of forwarding their contents after death, as is proved by their generally being found quite empty. The blood, in the case under consideration, was very fluid and black.

There was a statement in the Glasgow Herald at the beginning of the year, which was copied into the Medical and other Journals, that a gentleman inhaled chloroform, preparatory to having his toe-nail removed, by a surgeon in Govan, and expired almost instantly. As no account of this case has been given to the profession, we cannot discuss it; but there are two other cases which it is necessary to notice. The first of these occurred in the practice of Dr Barrier, at the Hotel Dieu at Lyons. The patient was a boy aged 17, on whom it was intended to perform amputation of the finger. A thin compress was placed on the face, and chloroform was dropped on gradually; in four or five minutes he was still speaking, and conscious of pain in the injured finger; a minute afterwards, he spoke again, and showed a little agitation. By this time, from one drachm and a half to two drachms had been poured on the lint, and it is to be supposed that a great deal of it went off by evaporation. The pulse had all the while been perfectly normal. All at once the patient rose suddenly, and threw about his limbs, but he was soon brought down again by the assistants. This excitement did not altogether last more than a quarter of a

minute, when it was found that the artery at the wrist had ceased beating. The cloth was immediately taken off from the face, which looked haggard. The heart ceased to beat, and no pulse could be detected. Respiration was, however, carried on, but soon became weak, slow, and ceased completely in about half a minute. On the employment of very energetic restorative means, the breathing began again, with a certain amount of vigour; the pulse, however, could not be felt, and the respiration soon ceased again. Artificial respiration was employed amongst other means. After death, the heart was found empty. The lungs were collapsed, and of a slate colour.

It was especially noticed in this case that the heart ceased to beat before the respiration entirely left off. Dr Sibson made last year a suggestion which is particularly applicable to this case, viz., that the blood passing from the lungs to the heart, and through the coronary arteries, is more highly charged with the vapour than that in any other part of the body, and may cause paralysis of the heart, before even general insensibility is induced. Some experiments of mine on frogs and other animals\* have shown that the vapour of chloroform has the power of acting locally on the heart, and suspending its action. It is therefore possible that, by means of very strong vapour, the blood passing through the lungs might be so impregnated with chloroform as to paralyze the heart, before the functions of the nervous system generally were abolished. To prevent this occurring, all that is required is that the vapour be uniformly and sufficiently diluted with air. In the case at Lyons, it appears that the patient got but little vapour into his lungs for several minutes, and then suddenly inhaled a considerable quantity, either from taking a deep inspiration, or some other cause.

The remaining case occurred very recently in Westminster, in a dispensary patient under the care of Mr Nunn's dresser. The subject of it was a labouring man who had his great toe amputated, on account of gangrene, following an accident. Half an ounce of chloroform was used on a handkerchief, with the effect of causing only excitement; insensibility was not induced; and a delay of two hours took place before fresh chloroform was procured. The second time the same quantity was employed as before. I understand that it was not all poured on the handkerchief at once, but was dropped on by portions; but of course it cannot be determined how much of the whole quantity was on the handkerchief at one time. In the account in the *Lancet* by Dr Arlidge, it is stated that, "after a period of excitement lasting two or three minutes, insensibility was induced, and the breathing, at first rather hurried, became now slower and rather sterto-

\* *Med. Gaz.*, vol. xlii., p. 415.

rous; the eyelids quivered on the approach of an object to the eye; the pupil became somewhat dilated; the pulse was at about 70, moderately strong. As soon as anæsthesia was produced the operation was proceeded with, and occupied not more than two minutes." I understand that the inhalation was continued during part of the time occupied by the operation. "At the close of the amputation no blood escaped when pressure was removed from the arteries. In the meantime the breathing became slower and less full, and a pallor with coldness diffused itself over the body, and showed itself in the lips, &c. The pulse receded in strength and frequency, and very shortly ceased at the wrist. The features assumed a ghastly expression, and everything betokened impending dissolution. A few respirations were noticed after the pulse ceased at the wrist, but in ten minutes (the time is probably greatly over-estimated) from the time of the inhalation respiration altogether ceased, and the chin dropped." The lungs and bronchial membrane were congested; the heart was rather large but flabby. There was an ounce of semi-fluid blood in the right ventricle, and a little in the left (artificial respiration had been employed). The contents of the head were natural.

This case shows in a striking manner the uncertainty of the employment of the handkerchief. In the first attempt to make the patient insensible, nearly all the chloroform was wasted by evaporation into the air of the apartment, and but little entered the lungs; whilst, on the second occasion, he at some moment got an overdose, and was no more. The coroner's jury returned a verdict of death by chloroform properly administered, which is tantamount to an assertion that the inhalation of it is attended with danger, however well managed, than which nothing can be more untrue. I have no desire to blame the parties in whose hands this unfortunate case happened, and who had sufficient precedents for the way in which they proceeded, but I cannot admit that chloroform is properly administered when it leads to the death of the patient.

There have been two accidents from persons inhaling chloroform whilst no one was present; but I shall confine this paper to the cause and prevention of accidents occurring during its professional administration. There was an alleged death from it, which must be fresh in the minds of the fellows of the society, to which I have not alluded. I mean that which occurred in the practice of Mr Robinson, but which, I believe, was not caused by chloroform, for the following reasons. In the first place, the patient could have had but an extremely small quantity of the vapour, since, according to the evidence at the inquest, the face-piece, of which Mr Robinson's inhaler consists, was not fitted to the face, but held at a little distance from it; the air would

therefore pass into the mouth and nostrils by the side, instead of going through the sponge it contained, and would consequently be but just flavoured with the chloroform. In the next place, the patient never exhibited any symptoms of the action of the vapour. He was talking one moment, and showing no signs of its effects, and the next moment his head and hand dropped, and he was dead. There was no convulsive action as in the cases in India and America, and that near Newcastle. No frothing at the mouth, as in the case at Boulogne, nor any exclamation respecting the strength of the vapour, such as, "I choke;" but, on the contrary, he was observing that the vapour was not strong enough, which, assuredly, he would not have done, if there had been a quantity of it in the lungs capable of causing sudden death. The decease of this patient seems to belong to a different class of deaths from those under consideration, viz. to those instances in which patients have suddenly expired when an operation was about to commence,—a kind of death from which patients are safe as soon as they become unconscious from the effects of chloroform. I look on Mr Badger's case as one of syncope, through fear of the operation or of the inhalation, concerning which he had been led to entertain apprehensions; and I believe that the syncope proved fatal from the fatty degeneration of the heart and the enlargement of the liver, which greatly encroached on the space of the thorax.

The death of a medical gentleman of Birkenhead, last year, was attributed to chloroform. He died of an attack of hæmoptysis, which came on two days after he had inhaled the vapour for the removal of a tooth. It is not surprising that, amongst the thousands of persons who have inhaled chloroform, one of them should have done so, a little time before he was about to have an attack of hæmoptysis. As the vapour has never caused hemorrhage from the lungs at the time of its employment, there is no reason to suppose that it could do so at an after period. There are other cases besides this, in which the death of patients, at periods more or less remote from an operation or confinement, have, by some persons, been attributed to chloroform. There is not time to consider these cases *seriatim*. I can only state generally my conviction, that the dangers, even of the mismanagement of chloroform, are chiefly confined to the moment when it is administered, and do not develop themselves afterwards.

The fatal cases we have been considering bear a considerable resemblance to each other. We have no particulars of the Glasgow case; but in four out of the other six the insensibility was produced very rapidly, proving that the vapour must have been inhaled of great strength from the first; and in the other two cases, as well as in these, the dangerous symptoms set in very suddenly.

From the sudden paleness observed in most of the cases, it is probable that the circulation was arrested at once, by the direct action of the chloroform on the heart.

The appearances met with on dissection do not differ from those that are found in many other cases, especially of sudden death. There was no particular congestion of the head in any of the five bodies that were examined. The lungs were much congested in two of them, and somewhat congested in the other three. The heart was quite empty in three cases, in which inflation of the lungs had been made after death. The blood was generally fluid, —in two of the cases very much so. I think that this is mainly attributable to the sudden nature of the death; the blood in the human subject very often remaining liquid after sudden death. In animals, such as cats and rabbits, the blood is generally partially, if not well coagulated, after they have been killed by chloroform; and if a portion is allowed to flow by a wound made just after death, this coagulates very firmly, showing that, even if this medicine have the power of interfering with the coagulation of the blood in the vessels, it is not by effecting an alteration in that fluid.

As regards the treatment for an overdose of chloroform, I believe that artificial respiration, carefully performed, is all that could be done; it would save the patient in any case in which the heart is not paralyzed; and, in such a case, I believe that nothing would be of service.

The points which I have endeavoured to impress on the Society are, that, whilst chloroform is perfectly safe when carefully regulated, by means of a suitable instrument, it is not so under other circumstances. In the inhaler which I employ, the compartment containing the chloroform is surrounded with cold water to limit the quantity of vapour taken up by the air, and the expiratory valve of the face-piece is so adapted as to admit additional air to any extent to dilute the vapour still farther.

It will perhaps be asked, how it is that accidents are not more numerous from the use of the handkerchief, if it be really so dangerous, for it is still employed to a considerable extent, though much less frequently, in this metropolis at least, than the various kinds of inhalers which are in use. I think that the skill and adroitness of medical practitioners are the chief causes of this; but I have heard of several cases, such as one that I lately related to the Society, through the kindness of Mr Henry Smith, in which the patient has had a narrow escape from impending dissolution during the use of the handkerchief.

ART VI.—*Researches on the Minute Structure of the Lungs in Man and the principal Mammiferous Animals.* By M. Le Dr ROSSIGNOL. A Memoir presented to the Academy at its meeting on the 4th January 1846. (*Memoires des Concours et des Savants Etrangers publiés par l'Academie Royale de Medecine de Belgique, Tome i. Bruxelles, 1847. 4to.*)

IN the first part of the sixty-ninth volume of this Journal, published in the beginning of 1848, was given an account of the minute anatomy of the lungs and the pulmonary vesicles, as ascertained by the labours of Dr Thomas Addison, Mr William Addison, M. Bourguery, Mr Rainey, and M. Alquié, up to the close of 1847. This account was preceded by such short comparative notices of the representations of previous anatomists, as might enable readers to understand what had been done by recent observers, to elucidate this department of anatomical knowledge, and to what extent they had confirmed, refuted, or rectified the observations of their predecessors, what they had added, and what they had modified.\*

Since that time the memoir of M. Rossignol has appeared in the Transactions of the Brussels Academy; and as it contains information which deserves to be made known, we shall here give the paper in an abridged form, yet sufficiently ample to communicate a faithful view of the researches of the author, and of the facts which he has ascertained.

M. Rossignol introduces his memoir by an account of the researches and opinions of Malpighi, Bartholin, Willis, Borelli, Duverney, Helvetius, Haller, and his successors down to Reissessen, Magendie, Everard Home, and Bazin. Of the researches and doctrines of most of these authors, we gave notices sufficiently ample; and these it is unnecessary now to repeat. The opinions and representations of Thomas Willis, we thought it superfluous to mention, for three reasons. First, they are avowedly an account of the observations of Malpighi, at that time recently made known. Secondly, because they are less accurate than that anatomist generally makes his descriptions. And thirdly, because, in consequence of their rather fanciful character, they had fallen under the censure of Malpighi himself, James Keill, and Cheselden. It is proper to allow, nevertheless, that, amidst some errors, the description given by Willis contains several points, which acquire considerable importance, since the views of the minute structure of the lung given by M. Rossignol have been made known. Under all circumstances, we conceive that it is proper to place the descrip-

\* Edinburgh Medical and Surgical Journal, vol. lxi. p. 193. Edinburgh, January 1848.

tion of Willis before our readers, in order that they may be enabled to estimate the true value of the descriptions of that anatomist.

The description of another author we passed over, because the statements were taken mostly from Malpighi, and they were more lengthened than our limits admitted. We refer to the account of the anatomy of the lungs given by Samuel Collins. This account is not noticed by M. Rossignol. To that description it may be proper here to advert; as, along with the facts adduced in the previous article in volume sixty-ninth, it will render the representation of the histological anatomy of the lung complete to the present time.

It is almost superfluous to say that, previous to the time of Malpighi, no account deserving the name of description of the structure of the lungs existed in the writings of anatomists. Adrian Spieghel may be taken as a fair average specimen of the best of these writers; and all that he says is, that the lungs consist of soft spongy substance, in order that they may be as light as possible; and that in the foetus, while still in the womb, where it does not require the function of respiration, they are firmer and more solid.

Malpighi was the first, we have formerly stated, who examined their structure with care, and explained it in a more intelligible manner than had previously been done.

Willis,—after noticing the description of Malpighi, and quoting that part of it, which shows that this anatomist represented the structure of the lungs, excluding the nerves and some vessels, with the branches of the trachea, to be an assemblage of *vesiculæ*, or minute bladders, that these vesicles are extended throughout the organ, and sinuated, yet are so situate and connected, that access is given them from the windpipe, and from one set of vesicles to another,—adds that he further ascertained, by microscopical examination, that these vesicles are connected and bound in masses by a sort of network, consisting of the branches of minute arteries and veins.

Willis then states that, besides these *vesiculæ* constituting chiefly the structure of the lung, Malpighi found a new apparatus; namely, that the mass of the lung consists of almost infinite lobules, each enclosed in a proper membrane, and which, provided with common blood-vessels, are attached to the ramifications of the windpipe. Of these lobules Willis gives a representation in his third engraving, showing them as large ovoidal or pyriform cells, each attached to the branchlet of a bronchial ramification. These cells are represented as attached by a narrow neck or peduncle to the minute bronchial tube, and bulging out considerably, so as to form expanded cells or chambers.



As this figure resembles rather the berries of a shrub or bush, for instance those of the barberry, or the bladders found in the ramifications of some species of sea-wrack, than any part of the animal body, it was in general regarded as the offspring of mere fancy, and has certainly contributed in a remarkable manner to destroy the credit of the other parts of the description given by Willis. It is, indeed, quite impossible to understand how Willis, who gives proof in all other subjects of having been a faithful observer, could have made himself believe, that this figure affords a correct view of the vesicles or cells of the lungs, as the terminations of the bronchial tubes, and of the relation between these cells and the tubes.\*

In another passage Willis gives the following commentary on the facts ascertained by Malpighi :—

“The bronchial tubes lead into more remote cavities, namely, numerous vesiculæ discovered (he uses the unfortunate term *inventas*) by Malpighi, which, indeed, are certain parts continuous with the windpipe, yet distinct from these, in so far as cartilages are entirely wanting; and they are separated from each other by longer spaces, which supply the parts of cartilages. The individual bronchial branches send off in all directions smaller divisions, the ducts of which, though void of cartilages, yet at certain intervals are contracted by bodies like ligaments; and the interspaces of these being filled with the air inspired in part form the vesicular cells. These tubes, indeed, may be not improperly compared to the colon of the mouse, the continuous cavity of which, as it undergoes contraction in various spots, presents the appearance of being divided into compartments.”

“Further, these vesicular tubes are provided on each side with short sinuosities or recesses, conjoined very closely like individual *vesiculæ*; and, in consequence, the aggregation of all the cells appears not unlike to a cluster of grapes. Of these a representation as exact as possible is given in plate third. These vesicular cells, that they may make contractile efforts for expiration, are also provided with muscular fibres, as is distinctly seen by the microscope. For occasionally a large supply of air requires to be drawn into the lungs, and, lest it should ever be wanting, to be in part reserved; consequently, besides the upper tubes, which act as a vestibule and halls, there are further required internal chambers of greater capacity, in which the air may be stored, and brought out for occasional uses.”

“Since, therefore, these vesicular cells receive a larger supply

\* Thomae Willis, *Pharmaceutice Rationalis, sive Diatribæ de Medicamentorum Operationibus in Humano Corpore. Pars Secundæ. Sectio I., cap. I., pp. 131, 132. Apud Opera Omnia. S. et O. Gerardi Blasii, M.D. Amstelædami, 1682. 4to.*

of air than that they can easily return the whole by one act of expiration; they are void of cartilaginous hoops, and these cavities are of themselves larger, that they may undergo further distension; nevertheless, in order that, when required, they may expel a large quantity of air, or eject matter requiring to be coughed out, being provided with muscular fibres, they contract more energetically, and eliminate their contents.”\*

It was unfortunate that, in order to elucidate the doctrine of Malpighi regarding these pulmonic vesicles, Willis presented this third engraving as representing the vesicles forming the lobules. These figures Borelli appears to have adopted, and founded on them certain objections. Malpighi, in reply to these objections, stated, that Willis had created in his fancy the figures representing the pulmonic vesicles and the course of the minute vessels around these bodies; and that Borelli having formed his conceptions from these fanciful figures, had consequently urged objections which were of no value.†

Thomas Bartholin adopted the description of Malpighi, and reproduced his figures so closely, that it is unnecessary to do more than merely to mention the fact. “The substance of the lung,” he says, “is an aggregation of very light and most delicate membranes which, by extension and situation, form an infinite number of orbicular and sinuous *vesiculæ*, like the *alveoli* or cells in honeycombs.” These vesicles are more distinctly seen, he adds, in frogs and tortoises, than in the human body.‡

Next comes Samuel Collins, who published in 1685 his *System of Anatomy, Human and Comparative*. His description of the pulmonic structure is evidently taken chiefly from Malpighi, and partly from Willis. It is given in the following terms:—

“The cylinders of air are branched through the whole body of the lungs, in many divarications, highly dilated in inspiration.

“These oblong tubes have many membranous appendages, affixed to them as so many outlets and receptacles of air, which, being big with it, do very much enlarge its perimeter.

“That I may give a more full history of these membranous cells, relative to the lungs as a machine of air, I will treat of their situation, connection, figure, origin, termination, substance, and use.

1. “These fine cells are seated every way near the bronchia, as so many appendants of them, and every cell hath a double passage, an egress from, and ingress into the bronchia, to give the air a free play in and out, upon inspiration and expiration.

\* Thomae Willis, *Pharmaceutice Rationalis*, &c. Pars Sec. p. 136.

† Marcelli Malpighii *Opera Postuma de Pulmonibus*. Londini, 1697. Folio, p. 12 et 13.

‡ Thomae Bartholini *Anatome ex Omnium Veterum Recentiorumque Observationibus quintum Renovata*. Lugduni Bat. 1686. 8vo, p. 418. Lib. ii. Cap. ix.

"2. These membranes or orbs of air are connected to the sides of the bronchia, as being a part of them, in reference to their continued inward membranes, and do participate in the same structure.

"3. They are adorned for the most part with an orbicular figure, as being the best and most capacious, wisely instituted by nature for the greater reception of air in inspiration, and the more easy exclusion of it in expiration.

"4. These fine round machines of air take their origin from the inward coat of the bronchia, as being alike in substance, texture, and use.

"5. So that these round cells, or membranous expansions take their rise from the inward recesses of air tubes, and are propagated through the whole substance of the lungs, and at last terminate into the coat, investing the ambient parts of the lungs.

"6. The substance of these orbs, which constitute a considerable part of the lungs, are chiefly membranous, and are a fine texture made up of numerous fibrils, passing in various right, oblique, and transverse positions, which give strength to these thin expansions, which would be lacerated when distended with air, were they not framed of many fibres, made in different postures, close struck, and curiously interwoven with each other.

"7. These receptacles of air are not only indued with membranous fibres, but fleshy too, which being annular, as those of the bronchia, are derived from them, and have a power of contracting these cells, to squeeze out the air in expiration, and throw the gross chymous or phlegmatic matter lodged in them into the bronchia in coughing, and afterward into the windpipe and mouth.

"Des Cartes conceiveth these vesicles of air, according to their variety of figure, to be consigned to a double use, the one to retain the inspired air, and the other to expel it. These sentiments of this author are more witty than profound, by reason, both the bronchia and these appendant cells of air are constituted by nature, first to be receptive, and after to be expulsive of air, and not to have a distinct office, at the same time to be some of them repositories of air, and others to throw it out in expiration.

"The pulmonary artery springing out of the right ventricle of the heart, and inclining towards the bronchia, is divided into a right and left trunk, out of which sprout on both sides an innumerable company of minute bronchia, making numerous inosculations, associates of the divarications of the bronchia, and are afterward dispersed into the lobules of the lungs, and being also companions of the bronchial artery and pulmonary veins, often touch each other's coats, and making great complications, at last encircle the small orbicular vesicles, and shading them with a fine network, swath their circumference to enliven and corroborate their fine compage.

“The pulmonary vein borroweth its rise from the substance of the lungs in small capillaries, encompassing the vesicles of air belonging to the bronchia, with fruitful ramulets, accompanying the arterial divarications, which often uniting and parting again after a small distance, make a kind of reticular plexes or meshes. These pulmonary veins answer those of the arteries almost in number and order, and much contribute to the fine contexture of small vessels, with which the orbicular cells are enamelled.

“The substance of the lungs is not only framed of numerous divarications of arteries and veins, but of nerves too, which take their origin from the *par vagum*, or eight pair of nerves, and are propagated, not only into the outward membrane, enwrapping the exterior parts of the lungs, but are also distributed into the body of the lungs, and associate with the pulmonary artery and vein, when they make their divarications over the surface, and over the orbicular vesicles of air.

“The lympheducts of the lungs are small tubes, clothed with most fine diaphanous tunics, which are affixed with minute membranes to the coats of the pulmonary vein, and then tend to the inward recesses of the lungs, and after pass out of them, and are at last inserted into the common thoracic channel, into which they discharge their lymph, the recrement of the blood (separated from it in the substance of the glands, which are very numerous in the lungs), and is afterward conveyed into the extremity of the lympheducts.

“If a greater inspection and deeper search be made into the substance of the lungs, the compage of them may be discovered to be in part composed of numerous lobules, of which every one is immured within a proper membrane, distinguishing them from each other by several thin walls, as so many different boundaries.

“These small lobes of the lungs are endued with divers kinds of blood-vessels, furnishing them with many divarications, accompanying the branches of the bronchia.

“The lobules may be discerned if the lungs be blown up and held up against the light, whereby some transparent interstices may be discovered, according to Malpighius, his observation, whereupon a gentle incision being made, and by tracing the interstices, the lobules may be found adhering to the sides of the bronchia and blood-vessels, from which they may be severed with a tender touch, and may be seen to be invested with proper tunics, and in truth are nothing else, as I conceive, but many systems of various vessels, confined within peculiar coats, for the better security of the numerous minute vessels, branched through the body of these lobules, and to keep them in a due order, for the better circulation of the blood.

The figure of these lobules is in some sort conical, as having

greater dimensions in the middle, and ending in an obtuse cone, resembling somewhat of a cypress nut.

"And it will be difficult to describe the situation, origin, and insertion of the lobules, by reason they are branched with the appendant vessels on every side of the bronchia, after the manner of the branches sprouting out of the trunk of a tree, and these ramifications are terminated into the outward surface of the lungs, which ought to be plain and equal; and sometimes these lobules are affixed to the extreme angles of the adjoining branches, that their due situation, union, and connection may be preserved, whereupon these lobules are seated sometimes in the lower region of the windpipe, and at other times in the sides or extreme parts of it.

"Next to the lobules, their interstices present themselves as the subject of our discourse, which are not merely empty *Aræ*, but are endued with extended membranes, sometimes parallel to themselves, and sometimes angular, which are not propagated from the ambient parts of the lobules (seated on their sides), but from their more inward recesses. And between these membranes filling up the vacuities of the lobules, many minute blood-vessels do run, and are derived from some, and are implanted into the adjoining lobules. Into these membranes interceding them, the air is received as into more large *sinus*, which have a mutual intercourse, that the received air out of one cell may be ejected into another by compression, so that these interstices interspersing the lobules, are nothing else but the membranous vesicles of air rendering the *aræ* transparent.

"These interstices distinguishing the lobules from each other, are most conspicuous in great animals, and do equal in dimension half a finger's breadth, and are conducive to the distinction and connection of the lobules, by whose interposition they are united; and when these interstices or membranous cells are distended with air, they do straighten the adjacent lobules, and assist the mixture of the blood with the chyme.

"And if any scrupulous persons be not fully satisfied, that there are any such lobules, or membranous *sinus* interceding them, I would advise them, for their better satisfaction, to view the lungs of divers animals newly killed, while they are warm; and then with glasses, if not with a naked eye, may be discovered a multitude of small orbicular vesicles distended with air, and may be farther discerned in the emptied lungs, cut through the middle, which then are less conspicuous, and may be better seen in lungs blown up and dried, by reason the round membranous *sinus* may be clearly discerned in the outward surface of the lungs, and in the dissecting of them may be clearly viewed the offspring of numerous hollow membranes distended with air."<sup>\*</sup>

<sup>\*</sup> A System of Anatomy, &c. By Samuel Collins. The second volume, Book II. Chapter xxviii. p. 797. London, 1685. Folio.

It appears from this account that Collins adopted the views of Willis regarding the figure of the vesicles, and their relation to the minute bronchial divisions.

Notwithstanding the objections against the description of Willis, his account of the minute anatomy of the lung, and also his figures, were copied by several eminent authors of systematic treatises. Thus we find the description of the vesicles or cells, and the terminations of the bronchial tubes, given by Willis, repeated pretty closely by Stephen Blacard, the author of a good treatise on anatomy, published at Amsterdam in 1686, and afterwards in an improved state in 1695. This description it is unnecessary to consider in detail, as one part of it is manifestly copied and abridged from that of Malpighi, and another from that of Thomas Willis.\*

The existence of *vesiculæ* at the extremities of the bronchial divisions is in like manner noticed, though in a very cursory manner, by Peter Dionis, professor of anatomy in Paris during the end of the 17th century, the great authority at that time in France, and indeed over the greater part of Europe.†

The descriptions of Stephen Hales, James Keill, Cheselden, Winslow, and the views and accounts of Senac, Helvetius, Haller, and Soemmering have been already stated in the sixty-ninth volume.

At a period long subsequent, and after the doctrine of ampullar enlargement at the ends of the bronchial divisions, and the existence of vesicles and cells, had been almost universally taught by anatomists, Everard Home, or rather Francis Bauer, presented in four plates of the Philosophical Transactions for 1827, which afterwards became the eleventh, twelfth, thirteenth, and fourteenth plates of the sixth volume of the Comparative Anatomy, views of the vesicles or air-cells of the human lung and the structure surrounding the cells, and, with the view of illustration, the air-cells of the lungs of the hare, and the large cavities of those of the tortoise. The principal thing which is learned from these views and the descriptive explanations is, that there are in the lungs two orders of cells, one superficial, large, the other deep-seated, small; the latter communicating with each other and the large ones; that, in the hare that is run to death, the large superficial air-cells are filled with coagulable lymph, and the small ones with extravasated blood; that the large cells at least are in shape irregularly spheroidal, or polygonal or polyhedral; that in their interior are open mouths of small cells, and open mouths of

\* Stephani Blacardi, Anatomia Reformata, sive Concinna Corp. Humani Dissectio. Ad Neotericorum mentem adornata. Lugduni Batavorum, 1695. Cap. ix., pp. 71 and 78. Blacard was a native of Middleburg in Walcheren.

† Petri Dionis, Anatomia Corporis Humani. Genevæ, 1696. Demonstratio Sexta, p. 287.

absorbent vessels ; and that, in the lungs of the turtle, the *bronchi* terminate, not in cells, but what Everard Home terms an open trellis work.\*

## CHAPTER II.—DIFFERENT MODES OF PREPARATION EMPLOYED TO DISCOVER THE STRUCTURE OF THE LUNG.

The lung in the recent state presents at its surface small cavities with membranous walls, which have no communication with the external air except by means of the bronchial tubes ; but it is impossible to penetrate further into the disposition of its parenchyma. Its chief use in this state is to serve for microscopical analysis of the different elements which enter into its constitution ; for the air, which it invariably retains in its structure, forms an impediment to vision ; and it is almost impossible to isolate properly the parts which it is desired to examine. This circumstance made the early anatomists feel the necessity of subjecting this organ to special preparations.

The methods employed may be reduced to two general heads, —desiccation of the inflated lung, and injection of the air-tubes.

1. *Desiccation after insufflation and retention of the air by means of ligature*, is the oldest method of preparation, the simplest, and the best. It preserves the normal conditions of the organ, and allows the observer to examine its interior.

By means of suitable sections, it is possible to trace the *bronchus* which penetrates into each lobule, and which I shall designate, for the sake of brevity, the lobular *bronchus*. It is possible, also, to examine the internal surface of the first branches issuing from it ; but it is nearly impossible to trace the subsequent divisions, or those of the second order, the walls of which are so thin and diaphanous, that they are not distinguishable from the neighbouring parts. The appearance of a section of a lung thus prepared, cannot be compared to any object better than that of very white bread, or a very delicate sponge. If a thin slice be placed beneath a lens or the microscope, there are seen only irregular cavities of all dimensions, sometimes sinuous, as Malpighi represented, and which seem to be provided with imperfect *septa* or partitions. This proceeds solely from the excessive transparency of the plates of the pulmonary parenchyma, so that certain walls, according to the direction of the luminous rays, necessarily escape the sight of the most practised eye, and the observer takes, for one single cavity, the assemblage of several, and, for a plane surface, that which presents superficial depressions.

This mode of preparation was employed by Malpighi, Helvetius, Bourguery, &c. Hence it is possible to understand the opinions which they expressed upon the *cellular*, spongy, caver-

\* Philosophical Transactions for 1827, p. 58 and 301.

nous, labyrinthine nature of the tissue, the arrangement of which they believed they had discovered.

2. *Injection of the air-passages with mercury* was employed principally by Willis, Reisseissen, and their successors. The mercury is allowed to flow into the bronchial tube of one lobule; and, by the course which it pursues, and the forms which it assumes, a judgment of the structure of this organ is formed. It is generally believed that this mode of preparation is superior to that previously mentioned. This is a mistake; though it enables the observer to understand the mode of division and the figure of the minute bronchial divisions nearest to the pleura, it does not afford the means of examining the interior of these tubes, nor the deep-seated recesses of the organ. Further, the mercury, by its tendency to assume the globular form, and by its weight, alters the shape of the pulmonic vesicles; while, by its opacity, which allows the observer to see only the first layer, it exposes him to the illusions of Willis and Reisseissen, on the mode in which the bronchial tubes terminate, on their relation with the *vesiculae*, on the number of the latter, and other points, as shall be seen in the subsequent chapter.

The injections which have been attempted with fusible metal, with tallow, and other substances, which, from a liquid condition, assume the solid form, have led to no advantageous result. By injecting one lobule with one of these substances, it is converted into a solid mass, the surface of which is marked by hemispherical globules, very difficult to be distinguished, and the interior of which presents the appearance of an amorphous substance.

Other objections yet have been urged against the two modes of preparation now specified. But these apply rather to the manner of making these preparations than to their nature. Thus it has been said that insufflation is liable to produce emphysema, and that mercurial injections have formed false passages. These results can take place only in instances in which the operator has proceeded with violence, or has neglected the most simple precautions.

Their greatest defect in my eyes is, that they are inadequate to determine the intimate structure of the pulmonary parenchyma. It is impossible to adduce a better proof of this, than the stationary condition in which this question has, for a number of years, remained, notwithstanding the recent and multiplied advances of histology.

[It seems doubtful from this passage, whether the author had perused the essay of Mr Thomas Addison, and almost certain that he has not studied that of Mr George Rainey.]

Desiccation of the inflated lung would be manifestly the best of all modes of preparation, if it were not, as has been said, encumbered



with the defect of preserving transparent the plates of the pulmonary parenchyma. This inconvenience Haller experienced. He speaks of colouring liquids in which he soaked the lung previous to inflating it. After many trials, the mode to which I have adhered, and which appears to me most advantageous, is simply, injection strongly coloured of the capillary blood-vessels, followed by insufflation and desiccation of the organ.

The selection of the material to be injected is important. It ought to be capable of circulating easily in all the capillary vessels, and filling them without transuding through their walls. The one which most completely answers these conditions is a mixture of oil of turpentine with about one-sixth part of varnish of cobalt and porphyrised vermilion. Of this last as much is introduced as the liquid can keep in a state of suspension ; for the more coloured the injection-matter is, the more perfect is the preparation. This injection, impelled slowly by the pulmonary artery, returns by the veins, after having coloured all the pulmonic parenchyma. If the last structure be sound, and if the mixture have been properly made, it never produces extravasation into the cells, or into the air-vessels. After injection, the matter contained in the larger vessels is allowed to escape ; an operation which does not diminish the coloration of the capillaries, and renders the sections to be made in the preparation more easily and more exactly performed.

Insufflation is performed with the ordinary precautions, that is, slowly, and waiting at intervals until the organ attains the limits of its expansion. As the ligature is tightened, the manipulation should always allow a little air to escape, in order to avoid the retrogradation of that which is retained. Lastly, to fulfil all precautions, desiccation ought to be accomplished slowly, and not by placing the preparation in an oven, as directed by M. Magendie.

Preparations made on a small scale, that is, on a small fragment of a lobe only, are always the easiest and the best. In order to attain the object proposed in this preparation, it is requisite that the pulmonic parenchyma be uniformly coloured, and appear as if painted carmine ; the plates of which it consists do not then escape from sight, and are not confounded with each other as in simple insufflation, but being rendered opaque by the injection, they project those lights and shadows which outline accurately the shape and the disposition of the cavities which they enclose. Further, the most superficial examination of a portion of lung thus prepared, is sufficient to satisfy any one of the little foundation for the opinions published on the spongy, cavernous, labyrinthine nature of the parenchyma of this organ.

Other advantages of this mode of preparation I have yet to specify. First, the capillaries of the pulmonary artery being alone injected, it is easy to recognise immediately, by means of the

coloration, the parts of the parenchyma, which contribute essentially to *hæmatoris*; and further, as the walls of the most of the air tubes do not participate in this injection, they are marked by their whitish colour from the red medium by which they are surrounded, which allows them to be easily distinguished and traced to their terminations.

This mode of preparation is not new; for there are more than one manner of injecting the blood-vessels of the lung. Nevertheless, I do not think that any one has ever applied it as a means of coloration, in order to discover the structure of this organ. Neither can this method be charged, I think, with altering the organization of the pulmonic parenchyma, since it preserves the normal conditions of that tissue in a greater degree than any other mode of preparation. It may be asked, indeed, if it do not place the blood-vessels full, as during life, in relation with the bronchial tubes, distended by air.

Before proceeding to the formation of a preparation of this kind, an indispensable condition is to select a lung perfectly sound. To forgetfulness of this precaution more than one erroneous opinion owes its origin; and it may without hesitation be said, that those anatomists who have seen the bronchial tubes communicate with each other by their terminal ends, made their observations on lungs attacked with emphysema. This is so much the more easily to be conceived, as, at the period at which appeared the most of the theories which admit this communication, the nature of pulmonary emphysema was almost unknown. At the present time (1846), it frequently happens that observers take for sound, lungs which have been attacked by the early stage of this disease; because simple inspection of the recent lung, as M. Andral well observes, is inadequate to enable the disease to be recognized, unless by those who have made it their special study. This change is one of the most frequent met with in the inspection of dead bodies; for it is often produced during the last moments of life; it complicates almost all the lesions of the respiratory organs; and, in short, it is an idiopathic disorder of considerable frequency. Further, it is only in cases of accidental and sudden death that the lungs are found to be completely free from it.

[It is proper here to observe that not even in all cases of sudden death are the lungs free from emphysema. There is one form of sudden death, in which there is strong reason to believe that the main if not the sole cause of immediate dissolution, is a form of emphysema developed in a very short time in persons apparently in the enjoyment of perfect health. At least, no other cause adequate to produce the fatal termination has been found in cases in which the body is inspected. Instances of this lesion taking

place under such circumstances have been observed by M. Ollivier and others.\* The lesion we have also observed personally. The lungs present the usual appearances of emphysema, especially in front, where they are pale-white and crepitating, with air effused into their filamentous tissue. The surface is sometimes, indeed generally, prominent and projecting, showing the marks of having been forcibly compressed by the ribs and thoracic walls. The blood is generally in such cases fluid, especially in many of the vessels of the lungs; and in most cases it is frothy. The chambers of the heart are distended with semifluid blood, very dark-coloured. Some of these instances of sudden death have been observed to follow after great and long-continued corporeal exertion, as dancing, running, playing at cricket, and similar laborious efforts. But in other instances this has not been observed to be the case.]

To discover the normal type of the structure of this organ, it is evidently necessary to examine the lungs of animals put to death in a state of perfect health. The lung of the ox, which has been so greatly commended for facilitating this inquiry, is really useful only for the separation of the lobules. The lung of the domestic cat, on the other hand, appears to unite all the most advantageous conditions for the analytical examination of the respiratory organs.

### CHAPTER III.—THE MINUTE STRUCTURE OF THE LUNGS.

The lung of mammiferous animals is an assemblage of a great number of similar lobules attached to the extremities of one common bronchial tree. The problem of the texture of this organ, therefore, is reduced to determine the structure of the lobule.

Certain carnivorous animals, as the dog, the cat, and the jackal, have no pulmonary lobules; but in them the lung is divided into a great number of lobes, which may be regarded as lobules, notwithstanding their considerable volume, and the complete investment in which they are inclosed by the pleura.

The lobules are completely independent of each other, as Haller, not Reisseissen, first demonstrated; and they can communicate with each by no other means than by the footstalk which they receive from the branches of the bronchial tree.

The form of the lobules is greatly varied. Placed along the course of the air-tubes and blood-vessels, on which they are moulded, resting on each other so as to leave no void space, they ought to terminate externally with surfaces plane, convex, or concave, sharp edges, and pointed extremities; they represent every

\* *Edinburgh Medical and Surgical Journal*, vol. xlv. p. 533. Edinburgh, 1835.

sort of irregular segments of sphere, cone, and polyhedral figure. With this great diversity in shape, corresponds the greatest uniformity in the fundamental part of their structure. The researches made by me in this respect comprehend the following three principal points:—

1. The examination of the shape, of the disposition, and dimensions of the cavities which receive the air, or the air apparatus of the lobule.

2. The mode of distribution of the blood-vessels.

3. The microscopical anatomy of the tissues which enter into the structure of the lobule.

#### ARTICLE FIRST. AIR APPARATUS.

##### § 1. *Analytical examination of its constituent parts.*

A. *Pulmonary alveoli and infundibulum.*—If we detach, by a sharp instrument, a thin slice of the surface of a lung prepared according to the process already described, and if we then examine it with a lens, by reflected light, we observe that the part situate near its margins is covered with minute cavities, nearly regular, of polygonal shape, separated by thin but entire partitions, the height of which is in general unequal to that of the width of these cavities.

If we then examine this same section in a point where it is a little less thick, we observe distinctly that the preceding cavities are enclosed by clusters of five, six, eight, twelve,—more or fewer,—within other cavities which are larger, deeper, more rounded in shape, and separated by thicker partitions. While examining attentively these last cavities, the observer perceives that they are not limited in elevation as the first-mentioned cavities, and that they become so much deeper in a place where the slice or section is thicker; he perceives at the same time that they diminish in diameter, or rather in calibre; for, while looking through their orifices, the eye is unable to seize at one time more than a small number of enclosed cavities.

To study these last objects, and to form of them a complete idea, it is necessary to behold them, when they are thus isolated, because their walls are untouched; then to examine them again at the thinnest points of the section. It then becomes evident that they are true hollow prisms, more or less regular, perpendicular to the pleural surface, having the base rounded, and of the same dimensions as the orifice, placed side by side to each other, and separated by complete partitions nearly of the same height. These prismatic cavities correspond to those which are seen through the *pleura*, and which in recent lungs appear to be rounded. They have been taken alternately for the blind ter-

minations of the ultimate bronchial ramifications, for small bladders attached to the air tubes, like grape-stones in a grape, for flexures of the labyrinthine canals, and similar objects; and these are the objects to which have been applied the names of cells, vesicles, by most anatomists, and even pulmonary capsules, by Lereboullet. These denominations are improper. Nothing bears less resemblance to a hollow sphere or bladder than these polygonal cavities, the ingredient orifice of which is as large as the base or remote end; and it is a misapplication of the term cell, in histology to employ it to designate an object so different. The name capsule is equally unsuitable, since in anatomy it indicates a cavity closed on all sides by a synovial membrane, or the fibro-cellular investment of an organ.

There are objects well known by every one which present perfectly the figure and the disposition of these cavities; these are the *alveoli* or cells constructed by bees. It appears to me, therefore, to be much more rational and useful to apply to these cavities the name of pulmonary *alveoli*, than to preserve for them that of *vesiculae*, cells, or capsules, the smallest evil of which is that they convey a false idea, and recal the remembrance of theories of little accuracy.

This being fixed, I continue the examination of the pulmonary section, as its complete analysis demonstrates one of the most important dispositions of the air apparatus. I have shown that near the margins of the section, that is to say, where it is thinnest, there are only pulmonary *alveoli* contiguous to each other, without marked traces of the large cavities, which, in truth, isolate them in clusters, more or less numerous, while in the thickest spots these cavities are very visible. This proceeds from the fact that the walls of these last, diminishing in thickness in proportion as they approach the surface of the lung, end by becoming no longer distinct from the alveolar walls, which appear thus to be their continuations. In order to obtain of this point satisfactory proof, it is sufficient to observe the pulmonary section at the spots where these sorts of inscriptions commence; it is then distinctly perceived, that it is the external sides of these clusters of *alveoli* which, without being a great deal thicker than the other sides, and, instead of stopping like them at a certain height, are continued indefinitely, increasing in thickness, and which, in this manner, constitute the large cavities. Thence it proceeds, that, at their origin, they affect a polygonal shape, though more nearly approaching the circular form than the *alveoli*. In proportion as they become deeper, the observer perceives, that their orifices at the surface of the section become more and more round, and are considerably contracted; so that they represent, with considerable accuracy, a hollow truncated cone, or a sort of funnel, the large

end of which rests on the *pleura*. In order to abridge the description, and to render it more intelligible, I shall designate these cavities by the name of *infundibulums* or funnels.

These *infundibula* are not all perpendicular to the pleural surface. The greatest number of them follow an oblique direction, all proceeding from the top of the pulmonary *alveoli*. After a short course, they are observed almost all to become united two by two, or three by three. This conjunction is ordinarily made between adjoining *infundibula*; and, in this case, it is effected by a sort of progressive attenuation of their common wall. Further, pretty often two neighbouring *infundibula* are observed uniting with a remote third one, which advances to them by passing in the manner of a bridge over the *infundibulum* by which they are separated. In this case, these two last *infundibula* follow an oblique direction.

The common opening, which is the result of several *infundibula*, always appears of smaller dimensions than the sum of their united orifices, but larger than each of them singly. It affects at first the form of a circumference with two or three segments; but, in proportion as it recedes from the point of conjunction, it becomes rounded, by being a little contracted, and very soon it forms the orifice of a tube perfectly cylindrical. In continuing the examination of these tubes, we observe that they also soon unite, two and two, or three and three; consequently their direction increases in obliquity in relation to the pleural surface. Nevertheless, the eye, aided by the lens, by penetrating the common aperture, may trace them sufficiently far on their internal walls, and perceive still, in those that are most straight, some of the pulmonary *alveoli*, which cover the base of the *infundibula*. In their course, the uniting tubes receive, on their lateral walls, at different elevations, several orifices of *infundibula*.

The intervals which separate these parts have yet to be examined; for the *infundibula*, in undergoing contraction, and afterwards uniting in tubes, which become less and less numerous, necessarily leave between them, at different levels on the section, void spaces, always increasing in size. Setting aside the vessels which traverse these spaces, the observer perceives that the narrowest intervals are formed by very small cavities in the shape of cups, or the *calyces* of flowers, the middle ones by *alveoli* more or less complete. Lastly, in the largest are found entire *alveoli*, enclosed in *infundibula*, similar to the preceding ones. From this it results that, at this distance from the lobular surface, there is a repetition of the anatomical disposition which has now been described. In other words, the external walls of the *infundibula* and of the tubes serve to sustain other *infundibula*. The only difference between them is, that the first rest on a surface more

or less plane, while the second have no other support at their large extremity except depressions and prominences.

This disposition, which shows already an amount of surface destined for *hæmatisation*, extensive in a very different sense from what is usually believed, according to the received theories, explains very well why a slice of lung always presents the same appearance, whatever be the place from which it is taken. Indeed, any section made on a lung which has undergone the preparation described, uniformly shows surfaces perforated by the orifices of cylindrical tubes, which leave between them void spaces variable in size, formed of *alveoli*, some complete, others incomplete.

When the human lungs are examined through the pleura, we cannot recognize the existence of *infundibula*. If it be wished to see them by their large extremities, it is requisite to remove this membrane; then by means of a fine needle, and under the lens, to detach the walls of the *alveoli* which cover their basis. The funnel-like figure of these bodies then becomes visible in an evident manner; and we observe on the wall opposite to the eye, sometimes however on the side, the narrow circular and single opening by which this cavity communicates with the rest of the organ.

In the lung of the dog, and especially in that of the cat, the *infundibula* are more completely developed, and they may be seen through the pleura, even by the naked eye.

In order to complete the previous conceptions on the air-apparatus of the lung, it is requisite, further, to proceed from the interior to the exterior of the organ; that is to say, to follow the divisions of the lobular bronchus, to the point where they form the *infundibula*.

**B. Mode of distribution of the air-ramifications.**—Each lobule, whatever be its volume, receives only one single bronchial division; at least I have never observed more than one in the lung of man and the principal mammiferous animals. But I have often remarked that, in voluminous lobules, and particularly those that are broad at the base, the bronchial tube furnishes at the point where it enters the lobule, one or two ramifications which are distributed to its lateral parts. I am thence led to believe, that these ramifications are the canals which have been taken by M. Bourguery for supplemental *bronchi*.

Whatever be the point at which it enters, the bronchial tube advances in a line more or less straight to the centre of the lobule; but on proceeding thence, its direction varies. When the lobule has only one single apex opposite to the point of origin of the bronchial tube, as in the conical form, it continues the original direction, following a pretty straight line, to its termination; but, if on the other hand there be two or three opposite apices, the

lobular *bronchus* having arrived within a short distance of the peripheral base, is divided into two or three branches, which are incurvated, in order to reach the apices, ramifying as they proceed. Lastly, when a sharp margin forms the opposite point, the *bronchus* is divided into two branches, which wind along it in opposite directions.

From its origin to its termination, the lobular bronchus furnishes, in all directions, branches which gradually exhaust it. The number of these branches, their point of origin, and their direction, are very varied, and depend on the extent and the shape of the lobule. In general, the more voluminous a lobule is, and the more sharp margins and apices it presents, the more numerous are the branches of the first order from the lobular *bronchus*. In the most frequent order they vary from six to fifteen. When the lobule has a conical shape in any degree regular, these branches proceed from the central tree in regular and decreasing succession, alternating and radiating like a star in all directions.

The divisions of the first order of the lobular bronchus retain for the most part a rectilinear direction to their termination. In their course they furnish several secondary branchlets, which issue from them at right angles; so that the opening of communications appears made on their walls, as if cut by a punch. Each of them then terminates, either by continuing the irregular direction, or by furnishing alternate ramifications as far as the margin of the lobule, or the apex to which it is distributed, either by dividing itself into two or three secondary branchlets, which proceed from the same point in a perpendicular or slightly oblique direction to each other, and to the bronchial division from which they issue. This dichotomous or trichotomous termination is the most frequent; it usually takes place when the bronchial branch of the first order has advanced two-thirds or three-fourths of the course which separates it from the exterior surface of the lobule.

When one of these branches is divided longitudinally into two nearly equal parts, it is observed that the terminal point is marked on the demicylinder which thus results by a sort of apparent dilatation or expansion, formed by the mouth of the tubes, which issue from it in an opposite direction. It is so much less easy to believe in the existence of an actual expansion in the calibre of the bronchial tube at this point, that we meet no traces of such expansion in branches of the first order, which terminate in alternate branchlets, that is to say, in progressive exhaustion; and that, on the other hand, we observe similar expansions, not only in the lobular *bronchus*, but also in all the air tubes which issue from it, when this *bronchus* or these canals terminate by dividing into two or three tubes, which diverge from the same point and follow an opposite direction. Notwithstanding this, M. B uergy admits



the appearance now described to be a real dilatation of the bronchial tube ; for he says, that this air-capillary terminates in a small irregular expansion, sinuous, elongated, single, bifid, or trifid, perforated in each compartment by one or more labyrinthine orifices, and at bottom meeting with one of those which is continuous with the canal of origin. These expansions, indeed, he adds, "are the sinuous *ampullulæ*, mentioned by Malpighi as intermediate between the windpipe and the vesiculæ."\*

The *bronchus* of the lobule, therefore, is divided into branches of the first order ; these again give origin to branchlets of the second order. It is the latter, and the branchlets issuing from them, which have been by M. Bourguery designated under the name of SYSTEM OF LABYRINTHINE CANALS. Some of the circumstances which have made this anatomist believe, that there exists in the pulmonary parenchyma a labyrinth, I have already mentioned ; and I have shown that the simple appearance of a section or slice of lung, prepared after the method practised by me, is sufficient to induce us to reject this theory ; because this slice is far from giving, as it ought then to do, the idea of a space all intersected by endless tortuous canals. On penetrating also into this pretended labyrinth, and following the air-tubes into their successive ramifications, as far as the *infundibula*, which form their terminal expression, I shall furnish the most direct proof of the slender foundation for his opinion.

In order to understand the arrangement of the bronchial tubes which I have described, the sections to be made on the lung are easily effected, and require no other precaution but that of directing the cutting instrument from the point at which the *bronchus* or its first branches originate, towards the apex or summit of the lobule which is diametrically opposite. But the bronchial tubes of the second order, having more delicate walls, ramifying from their origin, and forming bends always almost at each branchlet which they send off, require a much greater degree of caution in order to be suitably opened. The first object is to study to discover the direction of these tubes, by tracing them with the aid of the lens, by their open orifice ; the next point is to remove, layer by layer, the substance by which they are covered. When the walls are laid bare through a small extent, the operator raises them by the point of a small needle introduced within them ; and, by repeating this operation, he advances gradually towards the termination of the air-tube. The process is in some sense the manipulation of the sculptor ; but the cutting instrument ought to saw instead of pressing. With a little practice, the operator may follow in some seconds a bronchial branch or branchlet, in all its degrees of ramification down to the *infundibula*.

\* Gazette Medicale, 16 Juillet 1842. Extract d'une Memoire presenté a l'Academie des Sciences ; and Edinburgh Med. and Surg. Journal, Vol. lxi. p. 198.

Nothing is more varied than the length of these branches, the mode of ramification which they undergo, the number of their subdivisions, and the direction which these subdivisions pursue. They may, nevertheless, all be referred to two principal types. The first comprehends those air-tubes which are subjected to the mode of division by alternate ramifications; the second those which obey the law of dichotomous or trichotomous division. One example derived from each of these types will tend to facilitate the comprehension of the disposition of these ramifications.

*First*, the observer sees a bronchial branch of the second order, rising at a right angle, furnishing immediately after its origin a branchlet of the third order, then form a bend, in order to furnish a second branchlet, again bend, giving off a third branchlet, and successively to the extent of five, six, eight-branchlets, still continuing amidst these sinuosities its original direction towards the external or peripheral surface of the lobule, where it is finally exhausted. The divisions of the third order, arising at acute, right, or obtuse angles, terminate mostly in the centre of the lobule, a few at its external surface, and in the intermediate portions. Each of these furnishes, in a very short passage, several branchlets of the fourth order, and terminates by bifurcating at an acute angle. It is ordinarily at the extremity, and in the lateral parts of these branchlets of the fourth order, that the tubes uniting the *infundibula* terminate in small numbers. These tubes therefore constitute the fifth order of the division of the lobular *bronchus*; but often we observe in the same branch the mode of division carried a great deal further, and uniting tubes, forming at short distance from each other, sometimes the sixth, sometimes the seventh, the eighth, and sometimes even the ninth order of ramifications.

*Secondly*, in the type of dichotomous or trichotomous division, which is the most frequent, a branch of the second order is seen terminating after a very short course, and without giving off lateral branchlets, in two or three tubes, which issue from the same point at very open angles. Each of these tubes or branchlets of the third order presents the same disposition, and gives off divisions of the fourth order; these in their turn furnish divisions of the fifth order, and so successively. All these ramifications in separating at their origin, at obtuse angles, necessarily advance to different points of the substance of the lobule; the uniting tubes of the *infundibula* form in this instance, as in the preceding type, sometimes the fourth, sometimes the fifth, but most frequently the sixth, the seventh, and the eighth order of division of the lobular *bronchus*.\*

\* In this enumeration of bronchial divisions, it is requisite to understand a fixed point of departure. Thus I consider as lobular *bronchus*, or trunk of the central tree of the lobule, only that which extends from the entrance of the *bronchus* into the lobule, to the point where its decreasing diameter attains that of the first branches which it furnishes.

In other respects, whatever be the mode of ramification which this *bronchus* undergoes, it is observed that the number of these divisions and subdivisions is generally in proportion to the volume of the lobules. Thus, in the smallest lobules, we see uniting tubes of *infundibula* forming the fourth and fifth orders of division, while in the large lobules, as in those of the ox, almost all these tubes form the eighth and ninth orders.

It is observed, further, that the air-tube ramifications become so much more numerous and shorter, as they belong to a more elevated order of division; but their diameter is far from following the same progression. In the table of dimensions of the air-tube apparatus, it is seen that, between the trunk of the lobular *bronchus* and its first branches, between these and the branches of the second order, a decreasing progression of considerable rapidity is observed, while at the origin of these last, as far as the orifice of the *infundibula*, the diameter of the tubes remains nearly the same; in short, that it is rapidly increased from this point to the actual termination of the tubes, that is to say, to the end or bottom of the *infundibula*.

C. *Internal surface of the air-canals. Parietal Alveoli.*—When we examine by the aid of the lens the internal surface of the lobular *bronchus* and of its first branches, we observe through their semitransparent and whitish walls, ranges of *alveoli* which cover its external surface, exactly as we see through the *pleura* the *alveoli* which rest on its internal surface. If these walls be removed, or if a section falls at a short distance from these tubes, it is easy to observe, that these *alveoli* which surround them on all sides, are in like manner contained within *infundibula*. The same arrangement is observed in the blood-vessels, which invariably accompany these *bronchi*. It thence results, that the air-tubes and blood-vessels serve to support the *infundibula* in the lobules, in the same manner as they support the lobules in the pulmonary lobes.

The internal surface of the bronchial divisions is perfectly uniform, smooth, and glistening, wherever their walls can be distinguished, by the lens, from the subjacent parts; but in the bronchial divisions of the two last orders, and sometimes in those of the three last, the observer evidently sees when they are opened longitudinally, that the surface is lined, or, as it were, marked by a multitude of small regular openings, superficial, arranged side by side, and separated by thin partitions, entire, and of the same height, and which project into the interior of the bronchial tube. These small cavities, which I perceived first in the lung of the dog, and the existence of which I afterwards established in the lung of man and of the principal MAMMALIA, have a polygonal shape, the bottom rounded, and the same dimensions as the *alveoli* formerly described.

They cover the internal wall of the last air-tubes, in the manner of the cells which line the labyrinthine canals in the lung of birds, and constitute as in these last, part of the organ, destined particularly to *hæmotosis*, as is demonstrated by the great number of blood-vessels which traverse their walls. Indeed, in the preparations of the lung by coloured injection, the presence of these small cavities on the walls of the ultimate bronchial divisions gives to the latter a colouring of uniform red, so that they might be taken not for tubes possessing distinct walls from the rest of the substance of the lung, but for a species of subterranean galleries excavated in the parenchyma of this organ. It is impossible, therefore, to refuse to admit, as well by identity in shape as by analogy in function, that these cavities are genuine pulmonary parietal *alveoli*.

These *alveoli* rest by their bases on the bronchial wall, and their orifices are directed towards the central axis of the air-tube. Notwithstanding this disposition, they do not diminish the calibre or the capacity of the bronchial capillaries, because, at the point at which they appear, these capillaries are enlarged in such manner, that the free and equal margins of the *alveoli* form a continuation to the internal surface of the tubes which precede. Thence it results that, on examining a slice of lung a little thick, it is impossible to see the parietal vesicles of the tubes, which open at right angles to the surface of the slice; but, if these tubes are bifurcated at a short distance from their orifices, the sort of salient flexure or bend at the interior, which this bifurcation forms, enables the observer easily to perceive the *alveoli* which cover this surface.\*

At their origin, the parietal *alveoli* are few in number, and disseminated; but they very soon approach each other, to cover the whole internal surface of the ultimate bronchial divisions, and are separated only by thin partitions, which have in general less elevation than those of the *alveoli* which cover the base of the *infundibula*.

The point of division of the air-tubes establishes no line of demarkation between the parietal *alveoli*; they are seen to extend, without interruption, from one bronchial ramification to the following one. Nevertheless, when they have come near the *infundibular* orifices,—orifices, always smaller, as has been already said, than the calibre of the tubes of reunion,—it is observed that they suddenly stop, so that these circular orifices present a margin perfectly exact. Viewed on the side of the *bronchus*, by a magnifying power of from fifty to sixty diameters, and by reflected light,

\* In order to judge correctly of the disposition of the parietal *alveoli*, it is requisite to see them in tubes opened longitudinally in two parts nearly equal, and to allow the light to fall on them obliquely. By the refraction of the light through slices of uninjected lung, nothing is discovered; and the observer is exposed to numerous illusions.

these orifices appear to be a circular opening formed in the middle of a membranous partition, which constitutes the last wall or the common wall of the bronchial *alveoli*, and the *alveoli* of the *infundibulum*. If, indeed, we open the *infundibula* by a section perpendicular or slightly oblique to the surface of the lung, or even examine their internal surface, proceeding in the manner described above at p. 104, by means of the needle and lens, we perceive that the thin sharp edge of their orifice causes only a momentary interruption between the parietal *alveoli*; for it is seen, that they continue to line the concave walls of the *infundibula*, as they cover those of the tubes by which these are preceded. It may be remarked, at the same time, that the *alveoli* of the base of the *infundibula*, which are for the time designated by the epithet *terminal*, form the continuation of the parietal *alveoli*, that is to say, that their partitions become common, and that they equally project into the interior of these cavities. Lastly, it is observed, that the elevation of the inter-alveolar partitions gradually diminishes from the base of the *infundibula* to their orifice, a disposition similar to that which is presented by the pulmonary sac of the REPTILE family.

The number of parietal *alveoli* is much more considerable than that of terminal *alveoli*, since the latter occupy only the base of the *infundibula*, while others cover their convex surface or their outline, which is already more extensive; and, further, these cover the internal walls of the ultimate air-tubes. Each *infundibulum* contains from ten to twenty *alveoli*; the air-tubes enclose a much larger number.

The observer perceives, in the number and size of the parietal *alveoli*, differences corresponding to the principal periods of their existence, and which are not in exact relation with the modifications which the terminal *alveoli* undergo. Thus they are found to be smaller, deeper, and more numerous in infancy, when they occupy the two last orders of the air-tubes, than in adult age, when they line only the uniting tubes of the *infundibula*, and particularly in old age, when they in a great degree disappear.

On comparing, in this point of view, the lung of man with that of certain MAMMALIA, it is further practicable to establish some differences. In general, the parietal *alveoli* are less numerous in the lung of the calf than in that of man, and less numerous in the lung of man than in that of the horse. The lung of the cat, and particularly that of the dog, present a much larger number; for in these animals they occupy the three last orders of air-tubes, and are smaller, though of these tubes the diameter is equal to, and even greater than, that of the corresponding bronchial divisions in the lungs of the animals previously specified.

To the present time, the parietal *alveoli* have remained un-

known; or if their existence has been by some anatomists suspected, they have never been demonstrated and placed beyond doubt. It is even generally admitted at the present time, and stated in works of science, that the absence of these *alveoli*, in the lung of the MAMMIFEROUS Family, constitutes the principal character by which the lung of this class is distinguished from that of the BIRDS.

It is easy to understand the reason why these *alveoli* have escaped the researches of those anatomists who have studied this subject. The defect of the preparations which they have employed, has made itself particularly manifest in the study of this most delicate part of the pulmonary tissue. It has been already said, that, in lungs dried in the state of simple insufflation, it is almost impossible to trace the lobular bronchial divisions beyond branches of the second order. It is only necessary, therefore, to imagine the difficulties to be overcome, in recognising the divisions of the last order, in opening them longitudinally, and in perceiving, in their diaphanous walls, minute cavities, with partitions also diaphanous. It is in this manner, that, notwithstanding the habit which I have acquired, of examining them in lungs prepared by injection, I have succeeded only with great difficulty in distinguishing them in lungs simply inflated, except in parts coloured by melanotic matter. It may be necessary to say, that mercury, introduced into the bronchial tubes, is a method of preparation much less suitable still than the previous one for exposing the parietal *alveoli*, because it does not allow the observer to examine the interior of the air-tubes.

Among the anatomists who have suspected the existence of these *alveoli*, Willis ought first to be mentioned. It may be argued, indeed, that to these cavities this author adverts, when he states that the bronchial branchlets are contracted at intervals, and thus form a range of *vesiculæ* before terminating; (p. 90). This claim is to be supported, not on the description of Willis, which deviates too widely from the reality,\* but on the comparison between these strings of vesicles and the arrangement in the colon of the mouse or rat.

M. Andral, in his treatise on Pathological Anatomy, adds to the description of the air-tubes the following note:—"In the canals, however, we find at certain points, more or less marked, according to the subject, a species of plates, which rise from their surface, and on which is expanded a very delicate vascular network. These plates are analogous to the imperfect partitions which arise from the internal surface of the vesicular sac, which in frogs represents the lung. Ought not these plates," he asks, "in the *bronchi* contribute to the function of respiration?"

\* The passage is given at length in the third paragraph of page 90, where he compares the cells to those in the colon of the mouse or rat.

From this statement, readers might be led to believe that this anatomist beheld the interalveolar partitions. Unfortunately, the author says not, in which species of canals he observed these plates. The text to which this note is attached makes it further be presumed, that this is in *bronchi* of a certain calibre. These would in that case resemble closely a species of transverse semicircular bridles, which are sometimes observed in the *bronchi* of aged persons, who have long been labouring under moist catarrh. The bridles, which I have in some instances observed, have appeared to me to be formed by the transverse muscular bundles of the bronchi, which, being developed abnormally, and as it were contracted in their length, raise in different points the mucous membrane.

In other respects, this remark nowise affects the question stated by M. Andral, upon the part which these plates may perform in the act of *hæmatosis*. The anastomotic communications subsisting upon the membranous *bronchi*, between the capillaries of the bronchial arteries and the pulmonary veins, as shall be shown in the subsequent article, would lend some support to this opinion.

One author has recently expressed his views on the subject of the parietal *alveoli* in a manner much more explicit. M. Moleschott thinks that he has proved, in his dissertation,\* that Malpighi had intended to announce the existence of these *vesiculæ* in the following passage: "Membranæ istæ vesiculæ videntur efformari ex desinentia trachææ, quæ extremitate et lateribus in ampullosos sinus facessens, ab iis in spatia et vesiculas inæquales terminantur." These words, according to M. Moleschott, evidently represent the vesicles to terminate the bronchial tubes, not only at their extremities, but also on their sides or walls. In consequence of this explanation, he examines by the microscope thin slices of lungs, which have been dried in the state of insufflation. These slices are at first moistened by some drops of water, and then placed between two plates of glass. Viewed in this new condition by refracted light, he observes that they are from space to space perforated by irregular holes, more or less orbicular, which he takes for the pulmonic vesicles.

Accident, he states, was the means of his meeting once at first, and several times afterwards, slices of lung presenting towards their centre a loss of substance, a species of window-like opening, elongated in shape, the sides of which were marked with irregular sinuosities. This loss of substance could proceed, according to him, only from the longitudinal layer of a bronchial capillary, and the sinuosities evidently indicated the parietal *vesiculæ* described by Malpighi. From this he concludes that these *vesiculæ* are sometimes wide at base, sometimes petiolated, or

\* Moleschott de Malpighianis Vesiculis. Heidelbergæ, 1845.

contracted to a neck; that their shapes are varied, yet most frequently orbicular; lastly, that their dimensions are very irregular; and that in general they are smaller than the *vesiculæ* of the periphery of the lungs. According to this author, the bronchial capillary terminates in a similar vesicle, and before so terminating, it furnishes, on each side, a vesicle commonly larger than the others.

The delineation which he gives in support of this description represents one of these thin slices taken at random from the lung of a child. Examination of this figure, as well as the text of the dissertation, proves that M. Moleschott has not seen the parietal *alveoli*, notwithstanding every wish to corroborate by observation the sense in which he understands the language of Malpighi. If what I have said on the disposition of the air apparatus of the lung be read with attention, it is easy to be satisfied that the holes taken by the author for pulmonic *vesiculæ*, are nothing but the openings of bronchial capillary vessels, the area of which is altogether or in part included in the section. By means of this explanation we also account for the presence of the wide spaces which separate these holes, and which can never represent the thin inter-alveolar partitions.

As to the loss of substance mentioned by M. Moleschott, it is possible that it may be formed at the expense of the longitudinal layer of one portion of the bronchial capillary; but the recesses which the author considers as parietal *vesiculæ* depart from these so widely in figure, dimensions, and situation, that it is impossible to regard them in any other light than as accidental products, due to the method which he employs. On comparing with this figure the view which I have given of the bronchial tube, lined by its *alveoli*, and which has been drawn by the aid of a single lens, it becomes sufficiently probable that they are the inequalities of the margins of this canal or tube, resulting from the section, however clean it may be, which, viewed by Moleschott by the magnifying power of the compound microscope, and by transparent light, were taken for parietal *vesiculæ*. By a similar mistake the author takes the sinuosity in his sixth figure for the termination of the bronchial tube; for I conceive that I have proved that the *bronchi* never terminate in a *vesicula*.

The origin of all these mistakes is found in the method of investigation employed by the German anatomist. Manifestly it is impossible to choose a method more defective in investigating the structure of the air apparatus. Water imbibed by a thin slice of membranous *parenchyma* dried, infallibly impresses on the cavities which it encloses, modifications in shape, which expose the observer to the strangest illusions. These illusions are further multiplied by the effect of pressure of the slice between the glass



plates, and by examining it by means of refracted light, that is to say, by transparency. Lastly, we must add, that it is physically impossible, after having made numerous trials, to distinguish the parietal *alveoli* upon so thin a slice of pulmonary parenchyma, especially examined after the manner of M. Moleschott.

It is nevertheless requisite to understand the meaning of the expression of Malpighi above quoted. If this anatomist had been acquainted with the parietal *alveoli*, it appears to me that he would have mentioned them in other parts of his two letters; and this he has not done. On the other hand, what opinion is to be formed of his commentators, several of whom were contemporaries of this illustrious man, and who, nevertheless, make no mention of this anatomical fact, so highly important? Retaining, on the contrary, the literal sense of the passage, it may be inferred that Malpighi wishes only to say, that the pulmonary vesicles appear to be formed by the attenuated continuation of the windpipe, dilated at its direct extremity, and on the sides into sinuous *ampullulæ*, beyond which it terminates in irregular *vesiculæ*. This translation, which is made by most of the French anatomists who have quoted the Italian author, must at least have the advantage of leaving the passage in a state conforming to the text which precedes, and that which follows. It appears, therefore, that the interpretation given by M. Moleschott before it can be admitted, requires in its support additional proofs.

I know no other anatomist who speaks of the parietal *alveoli*, unless we refer, for instance, to Retzius, Bazin, and Muller, for the purpose of denying their existence in the lung of the MAMMIFEROUS Class.

## § 2. *Synthetical theory of the air-apparatus, deduced from the previous analysis.*

The air-cavities of the pulmonary lobule, therefore, are composed, 1st, of successive ramifications of the lobular *bronchus*, which ramifications proceed in all directions, centripetal as well as centrifugal, cross each other in all directions without ever anastomosing, become shorter and more numerous in proportion as they proceed from a more elevated order of division, and at length terminate in being suddenly dilated in the shape of funnels; 2d, of ranges of *alveoli*, which line the internal walls of the *infundibula*, and of the last bronchial tubes from which these issue.

Notwithstanding their number and direction, these ramifications would necessarily leave between them void spaces in the pulmonary lobule, if the *infundibula* did not fill these up, as has been shown, from the centre of the lobule, where they rest upon each other, and upon the principal air-tubes and blood-vessels, to its periphery, where, ranged side by side, and with their wide extremities directed exteriorly, they form the exterior surface of

the lobule. It is these external *infundibula*, which have been taken by Reisseissen and his followers for bundles of blind canals, similar to heads of cauliflowers; because the mercury, masking the internal parts, would show them only as minute hemispherical prominences at the pleural surface, occasioned by the pressure of the metal upon the base of the *alveoli*.

The distribution of the air-tubes, in the pulmonary lobule, though very varied, is subject, nevertheless, to a uniform law. This is, that each of them, with all the ramifications which issue from them and the *infundibula*, in which they are terminated, is destined to form a distinct part of its parenchyma, a sort of small or diminutive lobule contained within the first, and having with it no direct communication, a fact of which it is easy to obtain satisfactory proof. If, for instance, we open a lobular *bronchus* in the lung of the ox, and if we inflate one of the branches of the first order of this *bronchus* by the aid of a tube sufficiently tapered, we observe that the air enters only one part of the lobule which is perfectly circumscribed. On opening, afterwards, this branch of the first order, and by renewing the experiment on one of the divisions which it furnishes, which is always practicable by drawing the point of the blow-pipe along its parietes, and by blowing against them until we meet with the opening sought for, we ascertain that one single portion of the parenchyma, previously uninflated, is swelled up anew. Lastly, as the last term of this decreasing progression, arrives the bronchial tube, terminated by its *infundibulum*, which represents the smallest lobule, and also the fundamental lobule, of which the large lobules are only the multiplication, and which communicates with these only by means of the *bronchus*, which supports it. These experiments, repeated with mercury, furnish quite the same results as those which Reisseissen had already partly established.

The *infundibula*, or terminations of the bronchial ramifications, constitute one of the most important parts of the intimate structure of the lung. No anatomist has mentioned or even suspected their existence.

Each of these *infundibula* represents a small sac, of shape more or less conical, with the inner surface partitioned by numerous *alveoli*, having only one single opening of communication with the external air, and receiving only one single arterial branch, as shall be soon demonstrated. This is then, on a smaller scale, the image or the exact reproduction of the lung of the REPTILE class, and particularly of the BATRACHOID family. So that the lung of man, considered in this point of view, may be defined as the assemblage, the concentration of innumerable small lungs, similar to those of the REPTILES, and connected together by means of one common large bronchial tree.

The pulmonary *alveoli* are evidently the part of the organ destined for the function of *hæmatosis*, as is shown by the abundant apparatus of capillary blood-vessels sent to them by the pulmonary artery. By rising from the internal surface of the ultimate air-tubes, and by thus being placed on the passage traversed by the air, without obstacle, they appear in other respects peculiarly well adapted to expose the blood to the action of the air.

In preparations of the lung by coloured injection, made by the pulmonary artery, we uniformly observe that the bronchial tubes continue uncoloured to the point at which the parietal *alveoli* appear; a fact which sufficiently proves that these tubes, though capillary, do not perform the function which is generally ascribed to them, and that they are simple conductors of air.

The theory of the air-apparatus, which results from the facts now stated, resembles not any of those which have hitherto been promulgated, though it offers with them several points of analogy, which may be made apparent in a comparative parallel view. I mention here only some of its anatomical and physiological consequences. This theory shows, indeed, by the number, the shape, and the situation of the pulmonary *alveoli*, by their relations with each other and with the air-tubes, how simple is the mechanism, how admirable though hitherto misunderstood, which nature has employed to multiply in a small space the surfaces destined to accomplish the act of *hæmatosis*, yet preserving to them an easy communication with the external air. This theory explains the great disproportion existing between the number of the pulmonary *alveoli* and the number of the minute bronchial tubes, a disproportion which, at first view of a section of lung, strikes the observer, and which is not explained in the theory of Willis and Reisseissen. Lastly, it explains the reason why we find that, of the *alveoli* in the substance of the lung, the bases are as often directed towards the centre of the lobule as towards its periphery.

In a practical point of view, this theory, it may be said, combines the most favourable conditions. Has it ever been understood how *hæmatosis* can be accomplished according to the theories of the air-apparatus advanced by Malpighi, Helvetius, Boursery, and Addison? Without adverting to the mechanism for the renovation of air, which does not bear examination, is it not evident that, according to these theories, the air renovated being obliged to traverse the ranges of cells, or rows of labyrinthine canals, before arriving at the majority of the remote cells or canals, must lose in this passage its vivifying qualities, and in this manner render abortive the function of one part of the pulmonary parenchyma? On the other hand, in lungs constituted as I have described, the mechanism of the renovation of the air and the function of *hæmatosis* are in perfect harmony. The *infundibula*, or

terminal enlargements of the air-ramifications, are actual minute bellows, which aspire and expire the air during the movements of respiration, and move it in the air-capillary vessels; and besides, these *infundibula* receive all equally the mixed gas of atmospheric air, since they open separately in one common conduit, and as they are dilated and contracted at the same moment. In order to be satisfied that each *infundibulum* acts, in relation to the *bronchus* in which it opens, as a true bellows moved by the thoracic walls, it is sufficient to remember that it possesses the shape of bellows, and that its diameter is double and even triple that of the *bronchus*. If we consider further, that the ultimate bronchial ramifications are the terminations, at their extremities and on their lateral parts of several *infundibula*,—in other words, that they are the common tube of several small bellows,—we understand that there must be in the air-capillaries a current of gas greatly more forcible than can be imagined, according to the notions hitherto admitted on the structure of the lungs.

According to the theory of Reisseissen, it is impossible to explain the manner in which the *vesicula*, which, according to this anatomist, must form the terminal blind, or undilated extremity of the smallest conduit of the *bronchi*, can be put in contact with renovated air. We know that the quantity of air expired is about one-fourth of that which the lung contains after inspiration. It thence follows, that the air-ramifications, destitute of cartilaginous rings, as well as the pulmonic vesicles, augment and diminish successively by one-fourth of their capacity during the movements of respiration. Now, the *vesicula* being the most remote part of the *bronchi*, and, at the same time, the most minute ramification, we shall in vain search for those mechanical laws in obedience to which, the gas issuing from the *vesicula* during expiration, is not exactly the same as that which enters during inspiration, more or less mixed only with that contained in the capillary tube preceding the *vesicula*.

Other physiological phenomena, hitherto very obscure, are, with equal facility, explained by means of this new structure of the air-apparatus. For example, the sound which is caused by the lungs in the normal state during the movements of respiration, and which is designated by various names, as respiratory blowing, vesicular murmur, sound of expansion, &c., is generally ascribed to dilatation and subsequent collapse of the pulmonic vesicles. This, however, appears to me impossible; for there is neither in the shape nor in the situation of the *vesiculæ*, as described by Reisseissen, any of the mechanical conditions necessary to produce a sound during inspiration or expiration. This phenomenon, on the other hand, is very well explained by the ingress and the egress of the air of the *infundibula*. This fluid, indeed, on passing sud-

denly from a narrow place to one more ample, and the converse, necessarily exerts on the orifice of the *infundibula* a degree of friction, which is the cause of the sound specified.

I cite another example. Hitherto it has been impossible to determine the true cause which renders the respiratory murmur in children more intense than the same murmur in adults, and especially than that in the aged. It appears to me that the explanation is readily found in the table of the dimensions of the different air-cavities, in which I have compared the diameter of the orifices of the *infundibula* with that of their bases at the principal periods of life. It may be seen, indeed, that this orifice is only the third at most of the base in children, while it is the half in mature age, and still more in old age. This fact confirms the one previously given.

### § 3. *Indirect proofs in support of this doctrine.*

Examination of the lung prepared by coloured injection, is sufficient to show that the structure of the air-apparatus is such as has now been described. I may here, however, adduce in further support of this view, the evidence of diseases of the pulmonary parenchyma, analogy, numerous experiments, &c.; but a desire to abridge will allow me to mention only some of these complements of demonstration.

1. *Experiments.*—Injections with fusible alloys, wax or mercury, not only prove nothing against the theory now delivered, but well observed and correctly interpreted; they, on the contrary, confirm several of the facts on which this theory rests. The experiments of Reisseissen and Bazin may be said to show at first the general disposition of the lobule, and some of the peculiarities of the mode of distribution of the air ramifications which I have described. They further indicate a constant fact which these authors have not attempted to explain, and which nevertheless might have made them suspect the existence of parietal *alveoli*. Mercury introduced into the *bronchus* of a lobule deprived of air advances rapidly to its ultimate orders of division; but in order to cause it to penetrate to the *pleura*, it is requisite, as Reisseissen states, to urge the mercurial column by the handle of the scalpel, or to press the lobule between two plates of glass. Whence proceeds this impediment? It is certainly not from the calibre of the air-capillaries which it is obliged to traverse, since that is nearly the same as the calibre of the preceding series of tubes. Evidently it can be ascribed only to the uneven surface of these capillaries lined by *alveoli*; and which seems to be demonstrated by this irregular, jerking, and as it were spiral progress which the head of the mercurial column affects, in slowly traversing these parts. Having arrived at the fractional part of a milli-

metre from the pleura, this column is observed to become enlarged in the manner of a mushroom, and to require still more forcible pressure, in order to penetrate into the *alveoli* of the pulmonary surface. This enlargement, which experimenters have not remarked, and which is, nevertheless, easily established when the experiment is conducted with caution, I regard as a proof of the existence of the *infundibula*.

2. *Morbid state of the lung*.—Among the diseases which charge the pulmonary tissue, I shall refer only to emphysema. Upon the nature of this lesion modern physicians are as little agreed as were their predecessors. Both are divided into two opposite parties. Among the former, Ruysch,\* Baillie,† Laennec,‡ Louis,§ and others, maintain that the abnormal cavities presented in emphysematous lungs, are nothing but dilated pulmonic vesicles. Among the second, Storck,|| Van Swieten,¶ Magendie,\*\* Devergie,†† Piedagnel,‡‡ and Prus,§§ maintain, on the other hand, that these cavities must be ascribed to infiltration of air in the inter-vesicular cellular tissue. This discordance of opinion arises from the fact, that they have endeavoured to explain the origin of the anatomical lesion, on the assumption of different theories, on the intimate structure of the lungs.

Few physicians there are who, having examined with any degree of care emphysematous lungs, have not recognized the inadequacy of these two modes of viewing the subject. Nothing, accordingly, is more natural than to hear M. Bouvier declare that all that has been said and written on this subject requires to be subjected to new examination. Pathologists have never been able to explain, indeed, in a satisfactory manner, the numerous modifications which this disease causes to take place in the lungs, in their physical properties, and in their anatomical characters; and the mechanism of the production of this morbid state, and the relation of cause to effect in its different degrees, has been still less understood. Can it be otherwise, when we consider the vague notions, the incomplete theories which we possess on the structure of this organ?

In the *first* degree of *emphysema* we find the lung, especially at its surface, sprinkled with abnormal cavities, with walls more

\* Opera Omnia Obs. Anat. Chir. Amstelaed. Obs. 10, 20, 21.

† Morbid Anatomy of the Human Body.

‡ Traite de l'Auscultation Mediate. Paris, 1819.

§ Recherches sur l'Emphyseme. Memoires de la Societe Medicalé d'Observation. Paris, 1836.

|| Annus Med. Lect. Cad. 4. Amstelaed, 1757.

¶ Commentarii de Boerhaavii Aphorism, 1060. Lugduni Batav, 1745.

\*\* Journal de Physiologie. Tome vii. and viii.

†† Annales d'Hygiene et de Medecine Legale. Tome vii.

‡‡ Recherches Anatom. et Physiol. sur l'Emphyseme Pulmonaire. Journal de Physiologie, T. ix. p. 60. Paris, 1829.

§§ De L'Emphyseme pulm. Consideré comme cause de Mort. Paris, 1842.

or less smooth, and which vary in size from the volume of a grain of barley to that of a pea, as Laennec has stated. These cavities, which are generally globular in shape, represent with sufficient accuracy the figure of grape-stones, which Willis ascribes to the pulmonic *vesiculæ*; and the partizans of his theory have taken them for dilated vesicles. I shall not here repeat the well-founded objections which have been stated against this opinion; for its refutation is a sufficiently manifest consequence of the description of this pulmonary lesion.

When we examine these cavities by a powerful lens, in lungs injected, and in lungs prepared by simple insufflation, we see that they have only one single opening, by which they communicate, each more or less freely with one bronchial tube, and that they occupy exactly the same situation as the *infundibula* in healthy lungs. On observing with care their walls, and on passing from large to small cavities, we observe that the inner surface of the former is perfectly smooth, while that of the latter is covered with *alveoli*, more easy to be seen even than in the normal state, though their partitions are less elevated. The cavities of medium size indicate the passage from the one to the other; for we in these find entire *alveoli* and other incomplete, or rather united two and two, three and three, &c., by the common partitions disappearing, and the remains of which may still be recognised. The cavity which results from the junction of several *alveoli* into one, ordinarily affects a rounded figure, and this it is, doubtless, which, beheld through the *pleura*, has led to the belief in an incipient dilatation. I have often satisfied myself, by measurement, that the *alveoli* disappear in *emphysema*, without being sensibly augmented in capacity; but it ought not to be forgotten, that, in the normal state, this capacity always oscillates between certain limits, variable according to age. The inter-alveolar partitions are not uniformly obliterated by a gradual diminution in their elevation; the traces of actual laceration are in some instances observed. This takes place in lungs attacked with artificial *emphysema*, or with *emphysema* suddenly developed during life.

The principal character of the first degree of pulmonary *emphysema* evidently consists in dilatation of the *infundibula*, and in the disappearance or coalescence of the *alveoli* by gradual collapse, or by laceration of their partitions. With this dilatation of the *infundibula*, is sometimes associated that of the ultimate air-tubes, as is frequently observed in the *emphysema* of the aged.

It ought to be understood, nevertheless, how a cavity so small as an *alveolus* appears to be converted into another cavity of the dimensions of a pea, and sometimes even larger. This may explain, further, the new physical characters which the lung, attacked by this degree of the disease, assumes, and enable us to

understand the mechanical and physiological effects on the act of respiration.

The authors who represent *emphysema* to be caused by an infiltration of air into the inter-alveolar cellular tissue, have still fewer proofs than the preceding one in favour of their opinion. I will object to them only one single fact which has not yet been noticed; this is, that the walls of the abnormal cavities are covered by epithelium exactly as the pulmonary *alveoli*.\*

The examination of the other degrees of *emphysema*,—the *second* formed by the different modes of abnormal communication between the *infundibula*, the *third* by the rupture of the interlobular partitions, by the obliteration of the bronchial tube, and similar changes,—would allow me to explain the mechanism and the formation of these different morbid conditions, and to show how the *infundibula*, with or without their tubes of conjunction, are converted into these irregular cavities, separated by bridles or partitions, which the anatomical pathologists have described with great care. From these data, and from the disposition of the air-apparatus, would then result, as consequences easy to be understood, the different causes, sometimes of obliteration, sometimes of dilatation of the bronchial branches; those not less numerous of the retention of air within the *parenchyma*, of the diminution in the circulation of the blood, of the origin and of the formation of the different air-cavities, independent of the residue of the organ, among others, of those appendices which Baillie took for cysts secreting air, &c.; but this undertaking on a subject which a structure entirely new of the air-apparatus obliges me to view in a manner quite different, would demand too great development to be admissible here, and would encroach on a memoir which I propose soon to publish. What has now been said may be easily verified by observers, and to me it appears sufficient to show how important it is to understand correctly the true disposition of the pulmonary *parenchyma*.

3. *Analogy*.—The partizans of the theories of Malpighi and Helvetius have adduced as proof, the existence of cells in the lungs of REPTILES. Those who adopt the views of Willis and Reississen, not being able to find in the animal series an instance of lung formed solely by bronchial ramifications, have endeavoured to establish analogies with the glands, and particularly with the liver,—an organ the intimate structure of which is still less known than that of the lung. Lastly, M. Bourguery regards the disposition of the bronchial tree of BIRDS as a proof in support of his

\* In these considerations, I abstract from the disease designated by the name of interlobular *emphysema*, which constitutes a separate affection, possessing causes, symptoms, and prognosis different from pulmonary *emphysema*, properly so called, or parenchymatous *emphysema*.



system of labyrinthine canals. Manifestly it is impossible from these vague comparisons, imperfect and void of justice, to draw any conclusion; they prove only the impossibility of supporting an opinion which is not a correct expression of facts.

The air-apparatus is composed of two parts, as distinct anatomically as physiologically. One serves as a conductor of air; the other is particularly destined to accomplish the process of *hæmatosis*. If we consider it under this twofold view in the series of Vertebrated Animals, we see that as an organ for conducting air, it is represented in the REPTILES, at first by a simple tube terminated by a saccular dilatation (BATRACHIA), then by a tube ramified a small number of times, and the terminal branches of which, also dilated in the fashion of a sac, are intimately attached to each other, so as to form one single point. (CHELONIA). In BIRDS the bronchial ramifications become more numerous; but instead of terminating in shut sacs, they anastomose with each other at their extremities, and form a sort of labyrinthine network.\* In the MAMMALIA the bronchial divisions are multiplied a great deal more still, than in BIRDS, and are terminated separately in a dilated closed sac, after the manner of that of REPTILES.

The most important part of the apparatus, that namely to which is assigned the function of *hæmatosis*, is on the contrary identical in the three classes of the animal kingdom; for it uniformly appears under the form of cells, or rather of *alveoli*. The only peculiarity, is that in REPTILES these *alveoli* line the internal surface of the pulmonary sacs; and in BIRDS the walls of the tubes which mutually anastomose; while in the third class, they cover at once the walls of the terminal dilatations of the *bronchi* and those of the tubes, which furnish these dilatations. There is consequently in this respect in the MAMMALIA, not simple analogy with the two previously mentioned classes, but combination of the two fundamental types of intimate structure which they present; that is to say, an apparatus more complete, and sharing more in harmony than could have been believed with the organic development which this last class of the animal scale

\* This disposition of the bronchial tree in birds, which thus deviates from the common type, proceeds from the circumstance, that it is required to serve at once as an air-conductor for pulmonary cells, and for the air-sacs, accessory organs of the respiratory apparatus in this class. This perhaps requires explanation. It is well known that Nature always proceeds upon the principle of simplification or economy. The problem which it was requisite to solve, was to place the numerous bronchial ramifications in relation with the air-sacs, by a small number of openings. What means can be more simple, more suitable than that of anastomoses between the extremities of these ramifications? I have therefore reason to conclude, that the labyrinthine apparatus of the lung of the BIRD is a secondary character, depending solely on the presence of air-sacs, and that the true type of organization of the apparatus for *hæmatosis* in this class, is found in the presence of *alveoli* upon the bronchial parietes.

undergoes. This fact I regard as the most beautiful confirmation of the correctness of my discoveries.

§ 4. *Parallel with theories hitherto proposed.*

Agreeably to the views previously unfolded, it is easy to understand the differences and the analogies which subsist between the theory of the structure of the air-apparatus which I have here expounded, and the theories which have been hitherto proposed.

I think it is not possible to confound it with those of Malpighi and Helvetius, since it presents with these only one point of analogy, the polygonal (polyhedral) form of the pulmonary cells.

The theory which makes the nearest approach to it is that of Willis and Reisseissen ; for that presents an analogous mode of distribution of the air ramifications in the lobule, and of communications of these ramifications with each other. But from this again it differs completely as to the termination of the *bronchi*, the shape of the pulmonic *vesiculæ*, their number, their situation, and their connection. According to these anatomists indeed, and their followers, the final bronchial ramifications having become very short and attenuated, are closed at their free extremities, and thus form what they denominate pulmonary *vesiculæ*. According to my representation, the ultimate air-tubes are, on the other hand, terminated in a funnel-shaped dilatation, which is by no means a *vesicula*, but a small lung, having in its interior a certain number of *alveoli*.

These anatomists further admit, in consequence of an illusion resulting from the mercurial injections, that the *vesiculæ* are the ultimate prolongations of the terminal *bronchi*, and that they are attached to the extremities of these *bronchi*, as leaves to a stalk, or the grape-stones to the grape. I think, on the other hand, that I have demonstrated that they line the interior of these conduits in the same manner as the pulmonary cells of REPTILES line the surface of their respiratory sacs ; and in the same manner as the cells of the lung in BIRDS line the internal surface of the last bronchial tubes. In other words, I think I have demonstrated the fact, that the lung of the MAMMALIA forms, in this respect, no exception to the general law, and that there are nothing but parietal *alveoli* in the whole series of vertebrated animals with air respiration.

The theory of these anatomists establishes no anatomical difference in the air-apparatus, between the part destined to place the blood in contact with air, and that which is to act only as a conductor of this fluid ; for the one would be the consequence of the other without any line of demarcation. It is further generally admitted, according to this manner of viewing the pulmonic anatomy, that the *bronchi* serve for the function of *hæmotosis* as soon

as they proceed to the state of capillary canals. I have shown that the organisms of the lower animals (**REPTILES, BIRDS**) possess two anatomical parts, perfectly distinct (*alveoli*, canals), for fulfilling the two functions of the air-apparatus of the lungs, and that the **MAMMALIA** are not, in this respect, placed below the other classes of vertebrated animals.

As preparations made by pouring mercury into the tubes allow the observer to see only the *alveoli* which cover the bottom of the *infundibula*, they have overlooked those which cover the other walls of the *infundibula* and the internal surface of the ultimate air-tubes; whence it results that the number of the pulmonary *alveoli* is almost one hundred times greater than has been believed.

These anatomists have assigned this organ the form of a small petiolated bladder, or that of a short tube, little or not expanded at its free extremity; while I describe it as a polygonal *alveolus* more wide than deep, and the orifice of which is nearly of the same dimensions as the base. This discordance further proceeds from the use of the mercurial column, which, in resting on the bottom of the *alveoli* of the surface of the lung, urges it before it, distends it, and thus gives rise to the belief in the existence of minute bladders. Anatomists have been so much the more readily induced to admit the rounded shape of the pulmonary *alveoli*, because these bodies appear so in recent lungs, when examined by the naked eye or by a lens of feeble magnifying power. This shape, however, is only apparent; for if the depressions and furrows which separate the small hemispherical prominences produced by the injection of mercury or the insufflation of air, be examined by a high magnifying power, it is easy to perceive, that they describe the sides of a polygon, so much more marked, as the metal fills more exactly the *alveoli*, or as the quantity of air gives the lung a degree of expansion more closely approaching to the state of inspiration.

Besides the observation of the lung in the state of desiccation, another reason is favourable to my opinion on this point. This is, that the polygonal shape is really that which in the same space gives to the pulmonary *alveoli* the greatest extent of surface; that is, that this form bears a better relation than any other to the function of the organ. Lastly, the *alveoli* appear under this figure in the lungs of **REPTILES** and **BIRDS**, in which, by reason of the volume and the situation of these parts, they less easily cause illusion.

Several differences of opinion have arisen on the mode in which the *alveoli* communicate with each other. Malpighi, Helvetius, and their followers thought that the cells opened into each other as the cavities of a sponge or the cells of the *corpus cavernosum*. By Willis, Reisseissen, Bazin, and their adherents, it has been

on the contrary maintained, that the *vesiculæ* can communicate with each other only through the medium of the bronchial tube, of which they formed the ultimate prolongations. The parietal position which I have assigned to the *alveoli* shows that neither the one nor the other of these views is correct, though both are in certain respects true. The *alveoli* of the same *infundibulum*, though separated by complete partitions, communicate directly with each other, after the manner of the pulmonary cells of REPTILES, or the cells which line the same labyrinthine tube in the lung of BIRDS. But it is evident that the *alveoli* of one *infundibulum* do not communicate with those of the adjoining terminal dilatation, except by the medium of the bronchial tube which supports them.

§ 5. *Dimensions of the principal air cavities of the Lobule.*

Since the time of Home and Bauer, who first showed that there is a difference in size between the vesicles of the surface and those of the interior of the lung, many anatomists have spoken of this difference without endeavouring to estimate it in numbers; but in the dissertation of M. Moleschott, I find that it is stated at  $\cdot 02$  of one millimetre in favour of the vesicles at the surface of the lung. It is generally admitted also, that the vesicles at the apex of this organ are larger than those at the base. All these differences may take place in the morbid state; but in healthy lungs nothing analogous is observed. First, it must not be forgotten that there never exists an absolute equality between the *alveoli* of one region and another of the lung; everywhere between the nearest and between the most remote are found differences to the extent of several hundredths of millimetres. In order to compare in this respect one part of the lung with another, it is therefore requisite to determine the scale of the principal dimensions of the *alveoli* in each of these parts, and not to compare the size of some vesicles taken at random. By following this rational method, I am satisfied that all the *alveoli* of one lung are equal, that is to say, vary uniformly within the same limits. In the following table I give the averages of the principal terms of comparison. These averages have been formed after making a great number of measurements. It may be observed in general, that, from infancy to old age, the pulmonary *alveoli* constantly augment in capacity at a rate of progression nearly regular.

Table of the dimensions of the air-cavities.

1. ALVEOLI.							
	Maximum in 100th of M.		Minimum in 100th of M.		Means or averages in 100th of M.		Equivalents in decimal parts of Eng- lish inch.
Fœtus from 5 to 6 months	·05	...	·02	...	·03	...	·0017
Infants at full time which have not breathed, and others which breathed for some hours. Same dimensions.....	·07	...	·03	...	·05	...	·0019
Infants from 12 to 18 months	·14	...	·08	...	·10	...	·0039
Children from 3 to 4 years	·16	...	·09	...	·12	...	·0057
... 5 to 6 ...	·18	...	·10	...	·14	...	·0055
... 10 to 15 ...	·20	...	·13	...	·17	...	·0066
Adults { from 18 to 20 ...	·25	...	·18	...	·20	...	·0078
... { ... 25 to 30 ...	·28	...	·20	...	·22—·25	...	·0098
Men of ma- } from 35 to 40	·30	...	·20	...	·25	...	
ture age } ... 50 to 60	·35	...	·25	...	·30	...	·0117
Old men from 70 to 80.....	·40	...	·25	...	·33—·35	...	·0137
MAMMALIA.							
The calf.....	·50	...	·12—·15	...	·25	...	·0098
... horse.....	·20	...	·10	...	·12—·14	...	·0055
... dog.....	·20	...	·04	...	·10	...	·0039
... cat.....	·25	...	·08	...	·15—·17	...	·0066
... rat.....	·25	...	·16	...	·20	...	·0078
... goat.....	·15	...	·06	...	·12	...	·0057

## 2. INFUNDIBULA.

	Base.		Orifice.	Average.		Base.	Orifice.
	Base.	Orifice.		Base.	Orifice.		
Children of 3 years	·25—·40	...	·08—·14	...	·35	·12	·0137 ·0057
Men of 40 years...	·50—·85	...	·30—·40	...	·70	·35	·0274 ·0137
Men of 72 years...	·60—1·50	...	·35—·70	...	·85	·45	·0334 ·0176
Dog.....	·30 ·40 ·50	...	·15—·20				
Cat.....	·40	·70	·25—·30				

In the adult man the intervalveolar partitions have a thickness which varies from ·015 to ·03 of one millimetre at the level of their base. As they advance thence they gradually diminish in thickness to their free margin. The *infundibula* at the pleural surface are separated by partitions or walls, the thickness of which varies from ·03 of a millimetre to ·05 of a millimetre, in the lung of the cat.

The height of the alveolar walls of the base of the *infundibula* varies ordinarily from ·15 of one millimetre to ·20 and ·25 of one millimetre.

The dimensions of the different parts forming the air-apparatus, taken in a conical lobule of the lung of the calf, give in diameter:—

	Hundredths of one Millimetre.
The lobular <i>bronchus</i> at its origin, .....	120
A branch of the first order at origin, .....	80
A branch of the second order at origin, .....	40
A branch of the third order at origin, .....	35
A branch of the fourth order at origin, .....	35
Uniting tubes of infundibula, .....	45
Orifices of Infundibula, .....	35
Base of infundibula, .....	75
Pulmonary alveoli, .....	25

## ARTICLE SECOND. VASCULAR APPARATUS.

The vascular apparatus of the lungs is not less important to be studied than its air-receiving system. Independently of its bronchial arteries and veins, which establishes a circulation analogous to that which exists in the other parts of the body, the lung possesses the pulmonary arteries and veins, which constitute a second circulation, more considerable than the bronchial one, and devoted to the special function of the organ; the processes of aeration and *hæmatisation*. In relation, therefore, to its vessels, it is impossible to compare the lung to any other organ in the economy. The liver itself, though the termination of a particular venous system, possesses only one single order of efferent vessels, that is to say, only one circulation.\*

The capillaries of the bronchial and pulmonary vessels form two large networks, which have between them no continuity, and which occupy in the lung positions perfectly distinct and limited. The vessels which terminate in these two systems of capillaries, efferent or arterial vessels, are void of anastomoses, and are consequently completely independent. The vessels proceeding from these capillary systems, venous or efferent, have, on the contrary, between them numerous communications. These facts, completely opposed to what is generally admitted, are established by numerous experiments, and require a separate exposition, the situation of which naturally follows the examination of the distribution of the blood-vessels in the pulmonary lobule. In this examination I shall merely mention the disposition of the vascular trunks in the lobes of the lung, as this disposition is sufficiently well known to offer few interesting observations.

### § 1. *Distribution of Blood-vessels.*

#### I. *The Pulmonary Artery.*

The branches of this artery are uniformly attendants of the

\* In comparisons between the liver and the lung, the physiologist must not forget this difference, which explains very well those which subsist between the modes of connection of the capillary vessels, afferent and efferent, to these organs.

bronchial divisions. This law undergoes exceptions only in the ultimate air-tubes, in which the presence of parietal pulmonary *alveoli* causes and requires a disposition of the sanguiferous apparatus, which has hitherto been unknown, as these *alveoli* themselves.

One pulmonary lobule receives only one bronchial branch, and to one lobule only one arterial branch proceeds. The calibre of this last is equal, or even superior to that of the air-tube to which it is attached. The mode in which the artery of the lobule is ramified is a repetition, on a small scale, of what the pulmonary artery presents on a large scale in the lung. In proportion as the lobular *bronchus* furnishes its branches of first, second, third, and fourth orders, the lobular artery gives off, at the same level, divisions, each of which goes directly to the air-tube corresponding. In the lung the artery is united to the *bronchus* by loose cellular tissue; but within the lobule the arterial division is intimately attached to the *bronchus*, and becomes more closely adherent to it as ramification proceeds. In consequence of this close connection, and the situation of the artery beside the *bronchus*, it happens that the vascular divisions require to be incurvated around the bronchial tube, in order to proceed to the bronchial divisions arising on the side opposite to the artery. From this it results, that the bronchial branches are surrounded from distance to distance, that is to say, at their point of origin and of ramification, by demi-cinctures or vascular ringlets, which may be readily confounded with anastomotic arches. This fact explains the opinion given by M. Bourgery, who represents the air-tubes of the lobule to be enveloped from space to space, by irregular vascular rings, which rings he regards as the final expression of the arterial system, in the same manner in which he had previously considered, though erroneously, the air-tubes of the second and third order (labyrinthine canals of this author) as the terminal expression of the bronchial apparatus.\*

The lobular artery, in dividing in this manner, in the order of succession of the air-ramifications, decreases in calibre in a progressive and regular manner. I have shown that it was not thus in the air-divisions of the lobule, which retain nearly the same diameter on proceeding from the branches of the second order.

\* An afferent *arteriola*, says M. Bourgery, represents a stalk, of which the divergent branches are distributed like a cone or a tree. Two principal ramifications in separating penetrate into the inter-canalicular partitions. Beyond this they envelope the nearest canals by an equal number of polyhedrons, or irregular vascular rings, formed by one single vessel. The same disposition is repeated at intervals, all the canals being thus encompassed with annular vessels, interposed between their partitions, which terminate in each other at the tangent points, or *nodi* of intersection. At the other extremity the vascular rings recompose the branches, the inosculation of which forms the minute capillary veins.—Bourgery, *Traité d' Anatomie*, Tome iv. p. 59.

We also observe, as a consequence of this, that the artery, the calibre of which equals that of the bronchial tube at the point at which it enters the lobule, is not more than the fifth part of the tube, which it accompanies in the last orders of the air-tube divisions.

At the point at which the parietal *alveoli* appear, the arterial branch furnishes lateral branchlets indeterminate in number, and continues its course, as previously, along the air-tube to the *infundibula*. These lateral branchlets are ramified from their origin, are expanded over the whole external surface of the bronchial tube, anastomose with each other, and form a net-work, of which each mesh corresponds to the base of one *alveolus*, which it surrounds like a hoop or girdle. The diameter of the vessels which constitute this first net-work is from  $\cdot 02$  of one millimetre to  $\cdot 03$ . From this girdle or hoop arises a multitude of more minute branchlets, which are expanded over the walls of the pulmonary *alveoli*, anastomose with each other, and form, to the free margins of the *alveoli*, a second vascular net-work, the meshes of which vary from  $\cdot 02$  of one millimetre to  $\cdot 03$  in breadth. The vessels which constitute the meshes of this net-work are generally  $\cdot 01$  of one millimetre in diameter. Lastly, in the interior of each of these last vascular polygons is a third net-work, greatly more delicate than the preceding ones, for the size of its meshes does not equal the thickness of the vessels which form them (at least in the state of artificial injection), and these last have in diameter not more than one-half, one-fourth, one-eighth, and still less, of the capillaries of the second net-work, that is to say,  $\cdot 005$  of one millimetre to  $\cdot 001$ , which is about  $\frac{1}{225}$ ,  $\frac{1}{400}$ ,  $\frac{1}{800}$ ,  $\frac{1}{900}$  part of one line, according to Krause. We may regard as a fourth net-work, the external sides of the vascular polygons, which correspond to the free margins of the *alveoli*, for they communicate directly together. It is from this arterial or red-blood net-work, opposed to that which covers the base of the *alveoli*, and which contains dark-coloured blood, that arise in small number the rootlets of the pulmonary veins.

The minute artery which accompanies the ultimate bronchial tube is divided into as many branchlets as there are terminal *infundibula*. Each of these branchlets is disposed towards the *infundibulum*, in the manner already described as to air-tubes provided with *alveoli* on their walls. The base of each *infundibulum* is surrounded by an arterial circle, which furnishes internally the net-work at the base of the *alveoli*, and which anastomoses on all sides externally with the neighbouring circles; so that the pulmonary artery forms in truth only one single and extensive vascular net-work to each lobule,—a net-work which invests the exterior of the lobule, and is folded into the interior



in such a manner as to surround the two last orders of air-ramifications, and the external surface of all the *infundibula*.

From the preceding representation, it results that the dark-coloured blood follows in the part of the air-apparatus devoted to the process of *hamatosis*, an inverse course to that of the air, since it advances, so to speak, at its meeting, by rising from the base to the orifice of the *alveoli*.

The net-work of dark-coloured blood, which is expanded over the base of the *alveoli*, brings the distribution of the pulmonary artery close to that of the other arteries of the economy. Indeed, in the alveolar walls, as in all the organs into which only very delicate and numerous capillaries are to penetrate, the arteries form at their surface net-works which constitute sometimes membranes, such as the *pia mater* to the brain, the periosteum to the bones, &c.

The delicacy of the capillaries which traverse the walls of the *alveoli*, and which is such that the largest can admit the blood-globules only one by one, and the others the plasma only of the blood, shows the intimate contact which is established between the blood and the atmospheric air, and explains the reason why numerous ruptures may be produced in the alveolar partitions, without giving place to the slightest hemorrhagy.

The distribution of the ultimate divisions of the pulmonary artery, which I have now made known, is deduced in all points from observations made on injected lungs. It differs considerably from everything hitherto written on this subject. Malpighi had mentioned the presence of vascular network surrounding the walls of the vesicles; but it is requisite to go to Reisseissen to find information of any precision on the sanguiferous apparatus of the pulmonary vesicle. According to this anatomist, the vesicle being nothing but the terminal extremity of the bronchus, it receives like this last a single minute artery, which is expanded in delicate networks upon its walls, where it then gives origin to the rootlets of the vein which descends on the side opposite to the artery; that is to say, that, according to Reisseissen, each vesicle offers near its orifice on one side an artery, on the other a vein, which are ramified and anastomose in networks, down to the bottom of the vesicle. Observation is so much opposed to this manner of viewing the arrangement of these vessels, that I am led to think, with M. Bourgery, that it has been altogether the result of induction, or of the transference to the small scale of minute anatomy, of what is observed on the large scale in the substance of hollow organs with membranous walls.\* To me it appears that the partial observations which have since that time been made by several German anatomists, ought at least to have rendered

\* Bourgery, *Traité d'Anatomie*, T. iv. p. 57.

suspected the slender foundation on which the opinion of Reisseissen rests. Krause, for instance, observed, that the base of each cell is surrounded by an arterial circle, from which arise upon its walls numerous more slender branchlets, which form upon these a net-work extremely close, of which the meshes are not a great deal larger than the thickness of the vessels. I have above given the diameter which he assigns to these vessels.\* To this observation Berres adds, that each cell presents at its periphery ten or twelve hemispherical depressions, the bases of which are surrounded by meshes of a vessel having from  $\frac{1}{277}$  to  $\frac{1}{288}$  of one line.† This last fact appears to me incorrect; at least I have never met with any thing analogous; but I have often observed, in well-injected lungs, that the second net-work of *alveoli* makes a projection at the surface of their walls in such a manner as to lead to the belief, at first sight, that it circumscribes depressions. The observation of Krause is, on the contrary, very exact, and confirms one of the principal points in the distribution of the capillary blood-vessels which I have now described. Husehke, who records and admits this fact, appears not to have well understood it, as he still explains the circulation on the walls of the *alveoli* after the manner of Reisseissen; that is to say, admitting that one-half of the vesicle is devoted to an ascending venous current, and that the other half is to a descending arterial current;‡ while, according to the disposition of the vascular net-works admitted by Krause, there can be, as according to my theory, only one single current on the walls of the vesicle, a current centripetal to the axis of the air-tube. The disposition of the venous rootlets, which I am now to describe, will complete the demonstration of this interesting point in physiology.

The opinion of M. Bourgery appears to me to be the result of the process of investigation, which he employed. This anatomist having erroneously thought the artificial injections too coarse for the examination of the capillary vessels, satisfied himself with examining the natural injection by blood in the recent lungs of animals. By this method, however, it is almost impossible to see the capillary net-works of the walls of the *alveoli*; and this observer appears to me to have perceived only the minute arteries which accompany the air-divisions of the lobule. Nothing can therefore be more natural than to hear him say that he has been surprised at the considerable size of the capillary blood-vessels of the lungs.§

## 2. The Pulmonary Veins.

So far as we can trace the rootlets of the pulmonary veins, we behold them arise near the free margins of the *alveoli*, and thence

\* Husehke, Encyclo. Anat. Splanchnologie, p. 253.

† Ibid.

‡ Ibid. pp. 253 and 254.

§ Bourgery, Traité d'Anatomie, Tome iv. p. 60.

proceed obliquely between their partitions, to arrive at the external surface of the *bronchus*. I have explained, that a small artery formed to that alone the net-work which covers the base of several *alveoli*; in the same manner, here we find sometimes only one venous rootlet for the net-work of the free margin of several *alveoli*; but we observe that this net-work is gradually augmented in dimensions to the most manifest point of communication with the rootlet. In their course, otherwise very short, between the partitions of the *alveoli*, these rootlets are united in small numbers, and depart from the *bronchus* in a direction most frequently perpendicular to the axis of the air-tube, and describing an arc of a circle more or less marked. Externally, they constitute by union a venous trunk, which is attached to the air-tube on the side opposite to the artery. This situation is, nevertheless, far from being constant; for we find in the lobule numerous air-divisions accompanied only with one single attending arterial branch, and, conversely, venous branches which proceed solitary.

The venous rootlets issuing from the *alveoli* which line the bases of the *infundibula*, and sometimes also form the *alveoli* which cover the other walls, affect a peculiar disposition, which it is easy to demonstrate, at the surface of the lung. Instead of being conjoined to the small veins of the other *alveoli* of the same air-tube, they go to the surface of the lobule, where they are united to other rootlets of the same origin, in order to form a minute vein which then enters into the lobule along one of the partitions which imperfectly divide it, so that these small venous trunks carry back separately the blood, which has been reddened in the base of the *infundibula*. Sometimes we observe, over the whole extent of one of the surfaces of the lobule, only one single venous trunk, the rootlets of which issue from all the *infundibula* of this surface. This trunk then penetrates diagonally into the lobule, where it is united to a vein of the interior or a deep-seated vein. The superficial veins of which I have now spoken, are always placed outside the arterial net-work which covers the base of the *alveoli*, but within the proper membrane, or that investing the pulmonary lobule.

A third disposition of the superficial veins, not less important than the two preceding ones, remains yet to be noticed. This is the formation of venous trunks in the interlobular furrows. At the root of the lungs, and near their thin margins, are observed veins of considerable volume proceeding separately in the intervals of the lobules, immediately beneath the pleura, or at a short distance from this membrane. These veins, like the preceding ones, may be injected by the arteries and the pulmonary venous trunks. On tracing them in recent lungs, it is seen that they are formed by the conjunction of minute veins, which proceed each from the

pleural surface of a lobule. Here the venous rootlets of the surface of the lobule, instead of going to its centre diagonally, unite towards the interlobular furrow, with other minute veins arising in the same manner from a neighbouring lobule, and form one vein, which then runs in the interval between the lobules. After a course more or less lengthened, these superficial veins sink into the intervals of the lobules, and these go to the trunk of the nearest pulmonary veins, or they continue to wind along the pleural surface to their outlet, if they are placed near a venous trunk emerging at this surface, as is observed at the root of the lungs.

We have therefore to consider two venous systems in the lobule, and, consequently, in the lung; one deep-seated, formed by the veins which accompany the bronchial divisions, or which proceed separately in the interior of the lobule; the other superficial, less considerable than the previous one, produced by the minute veins of the lobular surface, which terminate, after a course more or less extensive, in the deep-seated veins, or which unite in part with the corresponding venous system of the neighbouring lobule.

The deep-seated veins, and the superficial veins which unite with them, form one single trunk, which issues from the lobule, sometimes at a small distance from the artery, in other instances adherent to it, or separated only by the lobular *bronchus*. These three situations, which appear to me to be equal in frequency, explain the reason why Reisseissen, Cruveilhier, and other anatomists have delivered on this point opposite opinions.

From the facts now detailed, it results, *1st*, that the pulmonary lobules are completely independent of each other, in regard to arterial and bronchial divisions, but that they are not all so as to the venous system; *2d*, that there are very striking differences between the pulmonary arteries and veins, as to their distribution and their relation in the lobule; for the former are invariably attendants of the air-tube divisions, while the latter appear subjected to no other law than to that of taking the shortest path to their destination.

The classical treatises on anatomy further teach, that the vascular tubes and the air-tubes proceed invariably in the substance of the lobes and pulmonary lobules in triple bundles, artery, vein, and bronchial tube. The disposition of the pulmonary veins which I have explained, shows that this order is not uniform, and that, further, there exists in the lung a venous system, namely, the superficial one, which has not been recognised. M. Bourgerie appears to be the only anatomist who has suspected its existence.\* According to this author, the air-tubes would be accompanied only

\* As to the vessels in themselves, says this anatomist, the bronchial canals are always accompanied by divisions of the pulmonary artery, and by no means by minute veins. These arise by the periphery of the lobules, while their arterial

by divisions of the pulmonary artery, and not at all by minute veins, all of which issue by the periphery of the lobules, in order to be afterwards united, traversing these same lobules in a diagonal form. "On the pleural surfaces, and in the interlobular furrows, we see," says M. Bourgery, "only their ramifications. Beyond this, the venous branches cross indifferently the direction of the other vessels and that of the lobules, to be collected in one large vein, which ascends also, without necessary connection with the arteries and the bronchial tubes of proportional volume."\* These observations of M. Bourgery confirm those made by myself, with this exception, that this anatomist gives them a different interpretation. According to him these peripheral veins constitute the whole venous system of the lobule; while they form only this part of the general system, which I have designated by the name *superficial*, as it is easy to be satisfied of the circumstance by injections properly made.

### 3.—*The Bronchial Vessels.*

The bronchial arteries are the nutrient vessels of the lung, that is to say, that they are expanded like arteries of the same description, into delicate capillaries in all parts of this organ. From this character, nevertheless, must be excluded the portion of the pulmonary parenchyma devoted to the function of hæmatosis, which is exclusively occupied by the capillaries of the pulmonary artery.

Besides the branches which the bronchial arteries furnish in the lung to the lymphatic glands, to the tunics of the pulmonary vessels, to the cellular tissue, and other parts, it is known that to each bronchial division corresponds an arterial ramification, proportionate to its volume. This ramification produces, in its transit, branchlets which are expanded into very fine capillary vessels in the bronchial walls, and form a net-work in the submucous cellular tissue. This mode of distribution is the same in the entire extent of the lung, and in the air-tubes of the lobule is found only the repetition of the arrangement which takes place in the air-divisions of the pulmonary lobes.

At the point at which the bronchial artery enters the lobule with the air-tube which it accompanies, being almost reduced to the capillary state, it furnishes branchlets, which are expanded into net-works in the interlobular and sub-pleural tissue, as Reisseisen has described it.

It is generally admitted, that the net-work which covers the branchlets enter them by the centre with the bronchial tubes. The minute veins, in order to unite with each other, traverse the lobules diagonally; the veins arising from them ascend separately. It is rarely observed that they appear united, in a certain extent, with the arteries and bronchial canals of proportionate volume.—Bourgery, *Opus Cit.*, p. 54.

\* *Ibid.* p. 60.

mucous membrane of the *bronchi* is continued without line of distinct demarkation, with that which the pulmonary artery forms on the walls of the cells or *alveoli*. This opinion appears to me to rest on no better foundation than that which admits the existence of large and numerous anastomoses between these two orders of arteries. I may say at present, that in the preparations of the lung, indicated as most proper for the study of the air-apparatus, we observe only the coloration of the air divisions of *alveoli*, and not at all that of the other bronchial tubes of the lobules, which ought, nevertheless, to take place if there were a continuity between these two orders of capillary vessels. In other respects this opinion appears to me to be rather the consequence of the theory of Reisseissen than the result of observation. If indeed we reflect, that the modes of preparation hitherto employed for the study of the lungs, do not permit the observer to behold the air-tubes of the different orders, sufficiently to distinguish the one from the other, and that if the observer had really seen the distribution of the arterial capillaries on the ultimate bronchial divisions, the parietal *alveoli* could not have remained unknown to this time, it will appear sufficiently evident, that the absence of all demarkation, either anatomical or physiological, admitted since the time of Reisseissen, between the pulmonary vesicles and the ultimate air-tubes, has been received by induction as to the sanguiferous capillaries of these parts.

The bronchial veins present in their principal branches the same disposition as the corresponding arteries. Their capillaries form, under the bronchial mucous membrane, very close net-works; a series of minute venous plexuses, the volume of which is considerable, especially in proportion to that of the corresponding arteries, as M. Bourgery has observed. This disposition of the venous capillaries is very obvious in the membranous *bronchi*, still more so in the air-tubes of the pulmonary lobules. The bronchial veins of the surface of the lobules are distinguished from the superficial pulmonary veins by their small size, by the numerous circuits which they describe, and by their being uniformly situate outside of the proper or enveloping membrane of the lobule.

Do the bronchial veins proceed all to empty themselves in the dark-blooded venous system, or have they, as Meckel thought, anastomotic communications with the pulmonary veins? This question, referring to the mode of connection and communication between the two vascular systems of the lung, shall be examined more seasonably in the following paragraph.

## § 2. *Mode of communication between the two vascular systems of the lung.*

Since the observations of Ruysch, Haller, Soemmering, and

Reisseissen, anatomists have admitted the existence of anastomoses between the ramifications of the bronchial arteries and those of the pulmonary artery. These anastomoses would be numerous, and sometimes large enough to equal one-fifth of one line, according to the statement of Haller.\* This fact, which appears so well established in anatomical science, was recently controverted by M. Natal Guillot.† This observer states that injections, impelled through the bronchial arteries, by no means return by the pulmonary artery, which proves sufficiently the absence of anastomosing communications between these vessels. His opinion, nevertheless, was not received as well founded. The most modern authors are satisfied with asserting that M. Guillot erroneously denies the existence of these anastomoses; and one of them, M. Huschke, adds, as a decisive argument, that, further, the capillary net-work of the bronchial mucous membrane is continued, without line of demarkation, with the net-works of the alveolar walls.

This twofold question of continuity between the capillaries, and of anastomoses between the branches of the two orders of afferent vessels of the lung, can be resolved only by means of artificial injections of the organ. In the preceding paragraph, I have already mentioned that this continuity between the sanguiferous capillary net-works is not a demonstrated fact; and to this conclusion I have been led by examination of lungs prepared according to the process employed for the study of the air-apparatus. This I have since verified by injecting only the bronchial arteries with the colouring matter specified, and allowing the lung to dry in the state of insufflation. In the specimen thus prepared, I have uniformly found that the capillary net-work of the bronchial arteries does not extend beyond the point at which the parietal *alveoli* appear in certain number; and I have never observed colouring matter in the walls of the pulmonary *alveoli*, excepting in one instance, in which, previous to injection, I had included in a ligature the trunk of the pulmonary veins.

As to the question of anastomoses between branches of a certain calibre, it is evident that, if they exist, injections impelled through the bronchial arteries, ought to return by the pulmonary arteries, and the converse. If, up to the present time anatomists have obtained in this respect opposite results, this proceeds solely from the different conditions in which they have performed their experiments. It is undoubted, that the results can no longer be the same, if we employ for injection matters suitable or unsuited, and if the lung be or be not in its normal state. For instance, can we regard as satisfactory experiments in which the injected

\* *Elementa Physiologiæ*, T. iii. p. 155. Lausannæ, 1761.

† Guillot *Journal l'Experience*, No. 255.

fluid has filled the pulmonary cells, and has returned by the *bronchi*? I think not; for we know that very liquid matters can easily pass from the air cavities into the blood-vessels. Neither is it possible to admit the evidence of the coarse injections which are effused by laceration of vessels. It follows, therefore, that the essential condition in the injection now under consideration, is the absence of all extravasation. Upon examining under this point of view, the experiments of Haller and of Reisseissen, it appears that the value, which has been assigned to them, they must lose. Haller employed as injection matter,—water, alcohol, milk, tincture of saffron, isinglass, mercury, oil of turpentine, and cerate. The composition of his injections Reisseissen mentions not; he merely says that he repeated the experiments of the great physiologist.\* But both record a fact common to all their injections, whatever be the vessel injected; this is the transition of the colouring matter into the pulmonary air-cells, and the air-canals. The nature of their injections sufficiently explains this facility of communication between the blood-vessels of the lung and the air-cavities. This Haller ascribed to the presence of absorbent vessels, and Reisseissen explained it by admitting that the capillary blood-vessels were provided with minute exhalant orifices opening into the air-tubes.†

It is sufficient to mention this peculiarity, in order to prove the necessity of subjecting again to examination the conclusions which these anatomists have deduced from their experiments.

When, for injecting the blood-vessels of the lung, a suitable liquid is employed, such as varnish, with a mineral colouring matter in the form of impalpable powder, extravasation is not produced, if the operation is properly performed, though this liquid forms one of the most penetrating injections. Proceeding in this manner, and considering as void the experiments in which the matter is effused into the *alveoli*, or into the air-tubes, I have invariably obtained the following results:—

1. In injections made by the bronchial arteries, the liquid returns in abundance by the pulmonary veins, in much smaller quantity by the bronchial veins, and no trace of it is found in the branches of the pulmonary artery.

2. By the pulmonary arteries, the injection returns altogether by the corresponding veins, and never by the bronchial arteries.

3. Lastly, the injection thrown through the pulmonary veins fills all the other blood-vessels of the lung, namely, the pulmonary artery, the bronchial arteries, and the bronchial veins.

From these facts it may be rigorously inferred, that no direct

\* Franz Daniel Reisseissen über den Bau der Lungen. Berlin, 1822. Folio, p. 16.

† *Hiant ex ultimis arteriæ surculis in bronchiorum tubos exhalantia ostiola. Ex his liquor, quem per arteriam aut venam injeceris, facile in bronchia exit, et rursus in bronchia injectus, etsi ægrius, in vasa penetrat.* Reisseissen, pag. 17.



communication exists between the bronchial arteries and the pulmonary arteries ; but that there are, on the other hand, numerous connections between the pulmonary veins and the bronchial vessels.

It is next a subject of inquiry to determine the mode of this communication. At this we arrive with sufficient facility by tracing on recent lungs, and on inflated and dried lungs, the different ramifications of the pulmonary veins, which have been injected by the aid of the bronchial arteries alone. This examination shows that a certain number of pulmonary minute veins have two sources of origin ; one proceeding from the net-works of the pulmonary artery, the other from the venous net-work or plexus, which covers the thin mucous membrane of the membranous air-tubes. In other words, the minute bronchial veins which proceed from the capillary net-work of the air-tubes, go to join in part the neighbouring pulmonary minute veins. This disposition is sometimes sufficiently manifest to be recognised by the naked eye on the membranous *bronchi*. It explains the reason why injection thrown through the pulmonary veins fills all the other blood-vessels of the lung.

#### ARTICLE THIRD. THE MEMBRANES WHICH FORM THE PARENCHYMA OF THE PULMONARY LOBULE.

Excluding the blood-vessels of which I have already spoken, the lymphatic vessels and the nerves which have not formed the object of my researches, the pulmonary lobule presents for examination ; 1<sup>st</sup>, a proper or investing membrane ; 2<sup>d</sup>, bronchial walls ; 3<sup>d</sup>, alveolar walls, and cellular tissue which unites all these parts together, and also the lobule to the *pleura* and the adjoining lobules.

1. *Proper or Investing Membrane*.—On a lung which has undergone commencing decomposition, or desiccation in the state of simple insufflation, it is sufficiently easy to remove in slips the *pleura* by which it is covered. Beneath is found a membrane impermeable to air, resisting to distension, and distinct from the alveolar walls which cover its internal surface. This membrane is formed by a very thin layer of elastic fibres, mixed with fibres of cellular tissue in nuts. In different morbid states it acquires remarkable thickness, as I have observed in the lungs of the aged. This tunic corresponds to the elastic reticulated membrane, which invests the pulmonary sac in REPTILES. The lung of BIRDS is void of it.

Without the presence of this membrane it appears to me difficult to explain well the great resistance which the pulmonary lobule presents to any distending force. Its existence has been recognized by Willis, Haller, Bazin, and Deschamps. The first three authors consider this membrane as of a fibrous nature. M. Deschamps refers it to the head of fibro-elastic tissue, which he

denominates *antitypien*.\* These authors, with the exception of Haller, erroneously admit that this membrane constitutes a sort of capsule or a general investment for each lung; for it is possible to follow it at all the surfaces of the lobule, to which it forms a complete envelope, independent of that of the other lobules. Of this fact it is easy to be convinced, by examining the lung of the ox.

2. The walls of the lobular *bronchus*, and of its principal divisions, offer in proceeding from within outwards, 1st, a layer of ciliated epithelium, the cells of which have the shape of elongated cones or pyramids; 2d, a mucous layer; 3d, a layer of longitudinal fibres, formed by expansion of the cords of elastic tissue so apparent under the mucous membrane of the large *bronchi*; 4th, another layer of fibres irregularly circular, which appear entirely similar to the muscular fibres of the membranous portion of the *trachea*. The air-tubes are further enveloped externally by cellular tissue with longitudinal and oblique fibres.

The cells of ciliated epithelium vary in dimensions according to the calibre of the air-tube which they occupy. In the *trachea*, they are from  $\cdot 06$  parts of one millimetre to  $\cdot 1$  in length; in the middle sized *bronchi* they measure not more than about  $\cdot 05$  parts of a millimetre; lastly, in the air divisions of the lobule they are only from  $\cdot 01$  to  $\cdot 03$  of one millimetre in length.

The bronchial mucous membrane is reduced in the pulmonary lobule to the state of a thin transparent layer, without trace of fibres.

The longitudinal fibres proceed in parallel direction closely pressed to each other. When they are isolated they describe arcs of a circle, or figures of eight (8); seldom do they take the undulating or spiral form. Their margins are obscure and perfectly smooth; they are bifurcated like elastic fibres, and acetic acid exerts on them no action as upon these elastic fibres. They cannot be compared to the smooth or organic muscular fibres, as has been done by Henle. This anatomist erroneously regards them as the transformation of the elastic longitudinal bundles into smooth muscular fibres.† It is requisite to observe that Henle examined them by subjecting to the field of the microscope a small bronchial tube quite entire; and that, by this method, error is nearly inevitable. To form a correct judgment of these fibres, it is requisite to isolate the layer which they form from the subjacent layer or that with circular fibres. The operation, though difficult on recent lungs, is sufficiently easy in lungs advancing to decomposition, or in preparations made by desiccation. On comparing

\* Deschamps, Memoire sur le Tissu Fibreux Elastique adressé a l'Academie des Sciences, de Paris, 1838.

† Allgemeine Anatomie.

then, the fibres of these two layers, we find between them well marked differences. Indeed, instead of presenting the preceding characters, the circular or external fibres are pale at their margins, granulated on their whole surface. They remain straight and rigid, though left to themselves. They are with difficulty separated from each other, and almost always present themselves united in minute bundles or fibrils, which offer at their surface and on their margins elongated globules, sufficiently similar to the *nuclei* of cellular fibres. Lastly, acetic acid dissolves not these fibres, but renders them paler and more difficult to be recognized. The granulations of their surface are not altered by the action of the acid, which makes the elongated globules more prominent, and renders them more distinct. These fibres, therefore, can be referred only to the class of smooth or organic muscular fibres, while the first belong evidently to the elastic tissue.

In the air divisions of the last orders, the muscular layer disappears, and the bronchial walls present then only longitudinal elastic fibres, covered externally with cellular fibres.

3. The *alveolar walls* are formed, 1st, by a framework of fibres, which leave between them void spaces or *areolæ*; 2d, by a transparent membrane which offers no trace of fibres, which covers the preceding frame-work and fills the void spaces.

The fibres of the alveolar walls are long, elastic, and incurved; they present the most of the characters of elastic fibres, though in a degree less marked than those of which I have already spoken. They surround the base and the free margin of the *alveoli*, intercrossing each other in different planes. Some are stretched obliquely from one extremity to the other of the *alveolus*.

On the alveolar walls, acetic acid exerts no action. When they are divided by the point of a needle, they present a clean surface without trace of filaments, and the borders of the fragments thus separated are turned or rolled in upon themselves.

In order to understand the formation of the alveolar walls, it is requisite to suppose the most internal elastic fibres of the bronchial walls separating themselves from the former in remote bundles, which raise to their level the transparent bronchial membrane. These bundles are then divided into thin groups, which cross each other, and thus form intercepted alveolar spaces.

At the point at which the parietal *alveoli* appear, the epithelium, which previously had been ciliated, becomes tessellated. Its rounded, and occasionally oblong cells, are, at their greatest breadth, from  $\cdot 003$  parts of one millimetre to  $\cdot 01$ . Direct observation demonstrates, in the alveolar walls, nothing analogous to the smooth muscular fibres admitted by many anatomists. The only sorts which are there observed, seem to be the continuation of the elastic fibres of the ultimate air-divisions.

Experimental physiology, the aid of which has been invoked in favour of the existence of muscular fibres, has, till the present time, furnished no positive information. The movements produced in the lung by applying galvanism to the divided pneumogastric nerves prove that there are muscular fibres in the bronchial walls, but by no means the existence of these fibres in the pulmonary *alveoli*.

M. Longet,\* and other anatomists subsequent to him, have imagined that they found a proof of the presence of these fibres in the formation of *emphysema*, after the division of these nerves. But these experiments, which I have frequently repeated, have proved to me that the emphysema, which ensues on division of these nerves, by no means derives its proximate cause from palsy of the pulmonary *alveoli*, as they suppose, but from a circumstance more decided, and to my mind more satisfactory. Division of the pneumogastric nerves invariably gives rise at first to bloody congestion, then to a form of pneumonia, which may proceed to suppuration, and which almost always causes death when both nerves are divided at once. The emphysema is only the consecutive and mechanical effect of this pathological alteration. It follows the progress of the pneumonia, is developed after it, and increases or decreases according as a portion more or less considerable of the pulmonary parenchyma is withdrawn from the function of respiration.

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ART. VII.—*On the Minute Structure of the Lungs.* By J. J.

PASCAL. Recueil de Memöires de Medecine par Begin, Vol. xxxix., p. 383.

THE account of the objects called pulmonary capsules is given from J. J. Pascal in the following manner.

J. J. Pascal has, in consequence of fine injections with fat substances and quicksilver, seen occasion to add to the hitherto known constituent parts of the human lung, another element, namely, the lung-capsule, uniting the pulmonic *vesiculæ* in small clusters, so as to form the pulmonary lobule.

These lung-capsules are to be distinguished from the interstitial ligamentous tissue, and likewise from the *pleura*; and, if the description of the author be correctly understood, they are to be regarded rather as the terminal ends of the trachea and *bronchi*. According to J. J. Pascal, the trunk of the windpipe is divided into *bronchi*, according to the pulmonary lobes; into bronchial cylinders, according to the separate divisions of the lobes; and,

\* Recherches Experimentales sur la nature des mouvemens intrinseques du poumon. Compte Rendu de l'Academie des Sciences, Tome ii. p. 501. Paris, 1842.

lastly, into bronchial plates, for the pulmonary lobules, held together by the pulmonary capsules. In the division of the trachea, the angles at which branches go off from each stem are always acute; these measure from 40 to 50 degrees, then 35, 30, and at length 25 degrees. In each bronchial bifurcation lies a lymphatic ganglion. The pulmonary lobules are distinctly quadrangular, and their longest side measures from between four and five to fifteen and twenty millimetres. These pulmonary lobules consist of distinct lobular clusters, sometimes oblong, of *vesiculæ* communicating with each other. There are no communications between the *vesiculæ* of different lobules. The periphery of the lobules is surrounded by branches of the pulmonary artery; and, from the centre of these lobules, arise the pulmonary veins. The air-cells are, therefore, only spheroidal excavations in the pulmonary lobules, which are enclosed within the pulmonary capsule. The *bronchi* have no ampullular terminations and canal-like elements.

ART. VIII.—*Observations on the existence of Free Carbon in the Human Body.* By JAMES PAXTON, M. D., Rugby.  
[Transactions of the Provincial Medical and Surgical Association, Vol. xvi., Part I., p. 51. London, 1848. Abstract.]

[In volume sixty-fourth was given a short account of the observations of M. Natalis Guillot on the presence of black matter in the lungs, as an instructive commentary on the papers of Dr Gregory,\* Mr Thomas Graham,† Dr G. Hamilton,‡ Dr Stratton,§ and the observations by Dr W. Thomson.|| On this subject, Dr Paxton of Rugby publishes, in the sixteenth volume of the Transactions of the Provincial Medical and Surgical Association, a short but interesting paper, which corroborates the views taken by M. Guillot.¶]

It is an important fact, that the only elementary substance as yet detected in organized bodies, chemically uncombined, is carbon. There is no part of the animal system in which it is so frequently met with as in the bronchial glands. The true cause of their blackness was first demonstrated by Dr Pearson.

Whenever there are pulmonary lesions of lengthened duration, or any mechanical interruptions to the respiratory functions, these glands become the repositories of carbon; the effect of which is to produce a degree of enlargement and induration.

\* Edin. Med. and Surgical Journal, Vol. xxxvi. p. 389. Edin. 1831.

† Ibid. Vol. xlii. p. 323. Edin. 1834.

‡ Ibid. p. 297.

§ Ibid. Vol. xlix. p. 490. Edin. 1838.

|| Medico-Chirurg. Trans., Vol. xx. p. 230. Lond. 1837. Vol. xxi. p. 340. Lond., 1838.

¶ Archives Générales, T. lxvii. p. 1. Paris, 1845.

The bronchial glands so affected are always enlarged, sometimes to twice or three times their natural size, and the black matter is infiltrated and deposited in their substance, so as to give the appearance of irregular winding lines, aggregated black spots, and sometimes uniform dark inky-coloured masses.

I have a specimen of carbonaceous bronchial gland enormously increased in its dimensions, and of almost scirrhus firmness, with the pneumogastric nerve adherent to it. It is not unfrequently the case that the bronchial glands are so loaded with carbon, that they are of an uniform inky colour. The extent of black discoloration bears some relation to the age of the person, to the state of the lung, and to the nature of the air breathed, particularly to the quantity of carbon it contained. The bronchial glands are always proportionably larger and blacker in those who are affected by chronic disease of the lungs.

In persons advanced in life, there is a disposition to carbonaceous retentions, more especially in the lungs; it indicates the period of senile atrophy at which they have arrived. A certain amount of black lines in the interlobular tissues, a gray or black web in the interior, occupying a greater or less space, with specks of various dimensions, and granules of the same appearance, constantly characterize the lungs of the aged. A chemical analysis by Guillot has proved this black matter to be exclusively carbon, in an exceedingly minute state of division. When this carbonaceous matter is blended together with the animal fluids, it becomes a *pigmentum nigrum*, corresponding to the secretion of the choroid membrane, and to the ink of the cuttle-fish. The colouring matter of all these substances is animal charcoal, identical with the animal charcoal artificially produced in its indestructible properties, and remarkable in its resistance to the action of the strongest nitric and hydrochloric acids or the caustic alkalies.

Here I wish to be understood that the carbon of the lungs in the aged is not always derived from the inspiration of impure air, but that it appears frequently, at least, to depend on insufficient combustion. A vesicular rhonchus is heard, if not in the ordinary breathing, yet constantly in attempting a deep inspiration. The dark parts are somewhat consolidated, while the remaining lobules are emphysematous.

Chronic pneumonia, whether simple or accompanied by tuberculous deposits, as a consequent affection, occasions accumulations of carbon, which are unequally insinuated in the pulmonary textures. It may be collected in large black masses, or it may be scattered in smaller isolated spots; at other times we find the black spots irregularly arranged in clusters. From such specimens, we shall be able to form some tolerable conception of the results of chronic inflammation in the production of carbonaceous

deposits. Hence vascular congestion has encroached on the air-vessels, and obliterated much of the areolæ, with even the ultimate ramifications of the bronchi. The air and blood are now no longer in contact, the extrication of carbonic acid ceases, and free carbon is detained in the lungs. We often observe large cavities at the apex lined by a carbonaceous pigment. During life, these cavities communicated with the bronchi; therefore, with the attendant cough, was gray expectoration proceeding from a mixture of this substance with the mucous and suppurative secretions.

Tuberculosis, or true *phthisis pulmonalis*, as it advances to a fatal issue, destroys the vesicular and tubular structures; consequently the aerial spaces become more and more limited, and, in proportion to the extension of the disease, the chemical changes required in respiration are equally limited. If the disease is protracted, similar depositions, from the causes just mentioned, take place; sometimes we perceive them centrally in the tubercle, but usually the carbonaceous depositions circumscribe the tubercle.

The effect of such black infiltration is induration of the part, with shrinking and corrugation. In certain cases, however, of carbonaceous lungs, there is no obvious alteration in form, the carbon being simply diffused through a congestive organization. One patient with this state of lung had vegetations of the aortic valves, which rendered the heart inadequate to the duties of propelling the circulation through the lungs; the process of oxidation was suspended; and decomposition of the blood followed.

It is not my intention to review every known phenomenon relating to the retention of carbon, but merely to glance at the fact of its being found in various parts of the animal system. We feel little surprise at the discovery of carbon in the organ destined to decarbonize the blood; but we are rather astonished at its appearance in the organs remote from the lungs, as in the mucous and submucous tissues of the colon. It is very irregularly distributed; for in some parts the colour is faint, while other parts are intensely dark, and the colouring matter is collected into little bodies about the size of hemp seeds.

I wish specially to draw attention to the consequences of carbonaceous affections of the lungs, particularly as there is a vague notion referred to by Hasse, that black deposit (denominated pseudo-melanosis,) is not only innoxious, but actually opposes a barrier to the encroachments of tubercles, and is "the almost unfailing concomitant of reparation of pulmonary disease." Another pathologist has asserted that "black pulmonary matter is altogether compatible with perfect health." Such views, however, are unsupported by the cases I have examined. On the contrary, I regard melanotic stains as marking a stage of degeneration, in which the vascular and aerial apparatus is most seriously

involved. So far as uncombined carbon invades the lungs, in the same proportion are the circulation and the respiration impeded.

We may, therefore, consider productions of black matter as no other than an unmixed evil. When added to tubercular mischief, it only increases the obstacles to the maintenance of the vital and chemical functions of the lungs, by further abridging the areolar surface, and by obstructing the escape of certain normal and abnormal secretions which might happen to be the attendants on the carbonaceous diathesis.

To what causes are such retentions of carbon to be attributed? Brockman thought it "an effort of nature to relieve the circulation of a surcharge of carbon." Heusinger, I imagine, approaches nearer to the correct theory, namely, a deficient elimination of carbon, and particularly of carbonic acid, assigning this reason for the opinion,—“Carbon being principally found in organs which ought to be the natural outlet of this element.”

In the cases I have studied, the retention of carbon arose under the following circumstances:—*1st*, During the reduction of animal power by lengthened years, when the pulmonary organs seem unequal to the task of expelling carbonic acid. *2dly*, Where the existence of disease prevents both the reception of oxygen and the escape of carbon from the lungs. *3dly*, Where the individual lived in a situation in which we might suppose there was insufficient oxygen in the atmosphere he breathed. It is not unreasonable to assume that every one of these causes, or a combination of them, may be sufficient to account for the retention of carbon in its passage through the vessels.

The next class of cases to which I shall advert are those in which carbonic acid, under certain morbid conditions, is decomposed on the surface, the carbon, uncombined, remaining on the skin. The extrication of carbon by perspiration is easily demonstrated by the colour of the linen when worn too long a time. Free carbon is also deposited on the epidermis of old persons, particularly under the nails, and “where free access of oxygen is impeded; for example, in the axilla and in the soles of the feet.”

The most extraordinary instance of the production of carbon I have ever witnessed, is that of a young lady, who, having been in ill health for some years, from an affection of the heart, has now exudations of pure carbon on the skin round the orbits and about the mouth. It is washed off in the morning, but it reappears in the evening. When wiped away with a napkin, we are immediately struck by the black sooty appearance. In a word, this exudation has all the properties of the smoke collected from the flame of a lamp.

A similar case is recorded in the *Medico-Chirurgical Transactions*. VOL. LXXI. NO. 180.

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*actions*, of transpiration of carbon from the forehead. [Vol. xxviii. London, 1845.] There is, therefore, competent authority for asserting that vital chemistry is capable of reducing one of the component parts of the body to its original element.

The series of cases just mentioned are entirely to be ascribed to an internal origin; the remote cause is organic, but the immediate cause is chemical, and constitutes a train of features attendant on the diversified diseases which interfere with the economy of respiration. Thus, the condition of the intestine is the result of an impediment to the reception of oxygen in the lung. On the other hand, the lung has become black and engorged from impaired forces of the circulation, owing to diseased valves of the heart.

The last series of carbonaceous affections which I shall have occasion to notice are those in which the introduction of carbon into the lungs is from external and incidental causes. I allude to the inhalation and absorption of molecules of carbon suspended in the air in the form of smoke. In manufacturing towns, the ordinary carbonaceous lung is very common. The professor of medicine in Queen's College, Birmingham, has never examined the body of an artisan who was exposed by his occupation to the respiring of an atmosphere containing carbonaceous matter, without finding his lungs more or less blackened by it. The effect of this during life is to interpose mechanical impediments to the functions of the lungs, in addition to the irritation excited by the presence of extraneous matter.

The museum of the Royal College of Surgeons of Edinburgh, and the museum of the University, contain various excellent specimens of this changed state of the lung. All these specimens mostly were obtained from the bodies of coal-miners. Occasionally the change takes place in the lungs of quarrymen and those exposed to inhale the air after explosions with gunpowder. These specimens look like lungs carved out of a piece of coal. I have in my possession a portion of miner's lung, sent from Edinburgh, and I do not exaggerate when I say that the whole pulmonary tissue is penetrated by minute atoms of carbon. Dr Ayres, who examined it, assures me that it contains a large portion of carbonaceous matter.

Perhaps evidence enough has been produced to prove that great damage is sustained by the pneumonic system, when, from whatever cause, carbonic acid is decomposed instead of being thrown off by expiration and the secretions. We ought to be no longer under the erroneous impression, that the retention of uncombined carbon has any other than ill consequences. I have also endeavoured to exhibit the equally injurious effects of breathing an atmosphere impregnated with smoke or coal-dust.

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## PART II.

### CRITICAL ANALYSIS.

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**ART. I.**—*Pathologia Indica ; or the Anatomy of Indian Diseases, based upon Morbid Specimens from all parts of the Indian Empire in the Museum of the Calcutta Medical College. Illustrated by detailed Cases ; with the Prescriptions and Treatment employed, and Comments Physiological, Historical, and Practical.* By ALLAN WEBB, B. M. S., Professor of Descriptive and Surgical Anatomy in the Calcutta Medical College ; formerly Professor of Military Surgery ; Surgeon to La Martiniere. Second Edition, in two Parts. Calcutta, 1848. 8vo. Pp. lx. and 340.

THE diseases prevalent in India may be considered in three different points of view. First are to be considered the diseases of the natives. Second, the diseases of European visitors and residents demand attention. Under the former head a natural question is, whether the diseases which the Hindoos and other natives present be the same as those which are seen to affect the human race in other countries placed under similar physical and geographical circumstances ; and whether these diseases are modified by the climate, by the habits of the people, by their diet and modes of living, and by all those other circumstances which are either observed or believed to modify the morbid element. Under the second head, all experience shows that Europeans in India become the subjects of peculiar diseases,—diseases affecting particular organs, and diseases showing a high degree of intensity. Thus we find that miasmatic and endemic fever is a common and a severe disease with Europeans in India ; that diseases affecting the brain and its membranes are also frequent and severe ; that diseased states of the liver proceeding often to suppuration and other forms of disorganization, as hypertrophy, kirrhosis, and analogous changes, are common ; that diseases affecting various parts of the alimentary canal, in the form of *gastritis*, gastric fever, dy-

sentery, diarrhœa, cholera, hepatic flux, are all more or less severe with Europeans; that rheumatic disorders and periosteal affections are not unfrequently general and severe; and that many Europeans suffer from affections of the bladder, prostate gland, and urethra.

It may become a just subject of inquiry, first, whether these diseases are peculiar to Europeans, and not very remarkable among natives; and, if so, what are the circumstances which render them thus peculiar and severe.

Any inquiry into these subjects is not only very difficult, but becomes in this case peculiarly complicated. Though India, stretching between 10° and 35° north latitude, must be regarded cosmically and geographically as a tropical and equinoctial country, yet it embraces so great an extent of surface, and is so diversified in climate, that, so far as the production and character of diseases are influenced by these circumstances, it becomes a task of great difficulty to present correct conclusions, and conclusions which shall be in all instances capable of application. The province of Bengal, extending between 20° and 30° north latitude, is on the one hand quite an equinoctial region; while the northern part of that province, bordering on and rising to the hill country, is much cooler, and presents less of the equinoctial character. The isothermal lines are depressed by the elevation of the region. Yet, even in the province of Bengal, with intense heat during the day, are alternated chilling cold in the early hours of the morning, and a depression of temperature sufficient to produce ice. The peninsula of India, which is traversed in its centre by the elevated chain of the Eastern Ghauts, presents at the coast, and along the low country, a great degree of heat, but in the mountainous parts is much cooler, and is liable to be well ventilated by cold gusts of wind. On its west coast, from the Gulf of Cambay in the north, to Cape Comorin in the south, runs the long mountainous wall of the Western Ghauts, having on the east, as a sort of elevated table land, the several provinces of Khandeish, Aurungabad, Beejahpoor, and Mysore. In the north of Khandeish are the Vindhya and the Sautpoora chains, running east and west, and closely connected with, if not continuous to the most elevated part of Hindostan. Lastly, the Himalaya chain, receding to the north about the 82d degree of west longitude, bounds on the north and on the east that immense tract of country which is intersected by the five rivers, and therefore named the Punjaub, and the direction of which towards the valley of Sind and the Indus shows, that it forms a separate hydraulic system from that of the Ganges. Northern India, indeed, may be said, as to its physical surface, to form two large hydraulic systems; one on the east, and trending nearly from north-west

to south-east, drained and traversed by the Ganges and its tributaries; the other on the west, trending from north-east to south-west, and traversed by the Indus and its tributaries. The line separating these two hydraulic systems passes obliquely from north-east to south-west through Delhi and Marwar, and is indicated by an elevated ridge of hilly country, in which are situated Pali, Ajmeer, and Jeypoor.

The circumstances now mentioned, with the numerous large rivers descending from these mountainous tracts, tend to impress on the climate of India peculiar characters, which cannot fail to influence the diseases observed to prevail both among natives and Europeans.

It is impossible to entertain any doubt that, in the diseases prevailing in a country so extensive, and in climates so varied, much has yet to be learned. Though knowledge derived from the meteorological history of other tropical countries may not be useless, yet it would neither be prudent nor safe to apply that knowledge, in order to account for the peculiarities of the diseases of India. Another mode of thinking very common among the inhabitants of cold and temperate countries, and countries with variable climates, is liable to lead into great fallacies on this subject, and to receive from the experience of India rather decided contradictions. It is commonly supposed, that diseases prevalent in cold and temperate countries are either not prevalent in equinoctial countries, or are there easily cured. This mode of reasoning is very often proved by the experience of India to be erroneous. Not only are diseases supposed to be chiefly prevalent in cold and temperate countries, and countries with variable climates, by no means wanting in India, but they are in many cases equally or more intense, more rapid in progress, and not less certainly fatal.

To throw light upon all questions thus arising, if not to answer all inquiries and determine all problematical points, the present work is undoubtedly well suited. Though the production of the Professor of Anatomy at the Medical College of Calcutta, yet it is avowedly founded on information derived from all parts of the extensive empire of India. The author, by his position, is enabled to obtain a great amount of authentic information from every quarter of that widely-extended and diversified region; and he has most assiduously and skilfully exerted himself to render the information useful to pupils, to professional brethren, and to the medical world at large. Neither can the work be regarded as that of one who, fixed in the metropolis of India, has not himself had opportunities of beholding the places and the people among whom these diseases have prevailed. Mr Webb informs his readers that he travelled as surgeon to the Bishop of Calcutta over the length and breadth of India; and in the course of these

journeyings, from Cape Comorin in the south to the Himalaya in the north, from the Sutleje on the west to the Burhampooter in the east, and from the Malabar Coast to the Straits of Malacca, he has seen not only most of his professional brethren in the three presidencies, but has visited all the great military and civil hospitals; and in this manner may be understood to have established channels for collecting information, which could not fail to add to the value of his labours.

Such were the facilities. The difficulties of prosecuting pathological inquiries in India on a systematic and continued plan are great and peculiar, and have repeatedly frustrated the expectations of accomplished and zealous men settled in that country. All know how much was lost in the premature death of Mr Twining; and the pages of the *Calcutta Medical Transactions*, as well as their fluctuating and changeful successors, can tell how much has been lost to medical science by the necessary casualties of Indian life. The following passage is a comment not undeserving attention, as an illustration of what actually takes place, and what must take place, in the course of every undertaking similar to the present. After stating that he feels thankful that he has been able to complete so much of his plan as the present volumes embrace, he adds the following acknowledgment:—

“Often during the five years in which this work has been in progress, it has been stopped,—the forms left idly to break up at the printer’s. For weeks together the destroyer has been among us. The friend in whose counsel I have one day joined at a cholera case, has been himself a victim before another sun had set; or again, the surgeon, whom I had left well and happy at dinner, is dead before night gives way to morning. It is difficult,—it is impossible to pursue with calm and equal thoughts the labours of literature and science amidst such scenes as these. The sheets have remained unfinished; the blanks have been torn off upon more than one occasion, to mark those very books as not my own, for return; in case I might never be permitted to resume the pen. Such is India.”

The subject first treated is the pathology of the blood. The introduction into the system of various poisonous agents, either by respiration or otherwise, may operate in causing various changes in this liquid and the vesicles, or the nucleated cells, of which it consists. Sulphuretted hydrogen may be inspired with the inhaled air, or it may be introduced from various changes going on in the system; and when introduced, Mr Webb thinks, its first effect is to invert the endosmotic action of the vesicles or nucleated corpuscles. They are not burst; there are no bloody discharges. This is a functional, that is, a dynamic disease.

In the symptoms ensuing on the bite of the rattlesnake, and we suppose the hooded snake or cobra di capella must be added,

in plague, and in *typhus gravior*, the vesicles, he says, have burst. The organism is destroyed. The vesicles are organically diseased. Typhus, therefore, and plague are organic diseases of the blood; while cholera and sweating sickness are examples of dynamic disease of that liquid.

As a good example of the former, the author adduces the instance of the Pali plague,\* a distemper with which, as well as its allied relative, the Kutch and Kattywar epidemic,† the readers of this Journal must be perfectly familiar. The district in which the Pali disease first commenced, is a geological tract in the north-west of India, which he represents as abounding in sulphur and sulphuretted hydrogen, consequently, he says, in *sesmaria*, that is to say, putrid or putrescent air. The disease itself, he maintains, exactly resembles the great plague or black death, which is said to have destroyed one fourth part of mankind. The traditions of the inhabitants, he adds, point to many outbreaks of this Indian pestilence previous to its last appearance in 1838. Four examples of the disease as it appeared in January 1838 are given; two in females, one aged 16, the other only 8; and two in males, one aged 37 years, another only 7. In the female of 16 and the male of 7 there were glandular swellings; but none in the other two cases. In the male of 37 there was a violent affection of the lungs, with repeated profuse hemorrhage, brought up by coughing, yet without pain in the chest. The female of 8 years, on the other hand, had dark-coloured vomiting of matter like coffee grounds. Dr Forbes states that, from the most diligent inquiry, and the results of what he beheld, he is convinced that, even in its present state, which is said to be milder than when it first broke out, four-fifths of the number attacked die.

Dr F. Forbes infers that, between this Pali distemper and the effects of the bite of the poisonous snake, there is so great a similitude as to admit of a parallel being drawn between them. To most European readers the effects of the bite of the poisonous snake are known through the medium of the interesting papers of Sir Everard Home; and from these we learn, that, soon after the poison is introduced, it causes swelling of the limb from general effusion of bloody serum, a sort of diffuse spreading or erysipelatous inflammation of the subcutaneous cellular tissue, with the formation of *phlyctenæ* on the skin, and eventually, if the patient survive sufficiently long, extensive disjunctive suppurations, followed by death. The first symptoms, however, are attended with disorder of the nervous system, and perhaps of the

\* Report on the Malignant Fever called Pali Plague, &c. By James Ranken, M.D. Edinburgh Med. and Surg. Journal, Vol. li., p. 231. Edinburgh, 1839.

† 1. Transactions of the Medical and Physical Society of Bombay. 2. Report on the Disease of Kutch and Kattywar, &c. Edinburgh Med. and Surg. Journal, Vol. lv., p. 194. Edinburgh, 1841.

blood and vascular system, so great, that death often ensues in the course of a few hours, or at the longest, three or four days. In one of the cases given by Sir Everard Home the patient survived the bite eighteen days, from the 17th of October 1809 to the 4th of November, when death took place; and, during this period, the symptoms, which were probably to be regarded as secondary, or dependent on the reaction of the system to resist the effects of the poison after absorption, were chiefly those which are observed to take place in diffuse inflammation. The appearances found on dissection were exactly those characteristic of diffuse or disjunctive inflammation of the cellular membrane.\*

Very different was it in the instance of a man bitten by a rattlesnake at Rouen in February 1827. This person was an Englishman, aged about fifty-one, who had brought from London three rattlesnakes in order to exhibit them at Rouen. Travelling from Havre to that city during the night, one of the animals had died in consequence of the nocturnal cold, and was removed from the cage in which they were kept by means of a pair of tongs. The other two, which seemed very languid, were brought with the cage near a stove. One seemed to show signs of life, and the keeper, Mr Drake, lamenting his loss, opened the cage and took the reptile by the tail, and at the distance of some inches from the head. Instantly the apparently torpid animal darted one of its poison-fangs into the posterior and external part of the left hand near the back of the thumb. Notwithstanding this bite Drake continued to hold the serpent, and received a second wound on the palmar surface of the same hand, at the moment when he was replacing the animal in the cage. This took place at half-past eleven in the forenoon.

Mr Drake washed his hands in iced water, and, three or four minutes after the accident, he tied a ligature above the wrist. He was seen, 10 or 12 minutes after the infliction of the bites, by M. Pihorel of Rouen, who found him in a state of great apprehension and agitation, pale, his face and neck covered with a cold sweat, and his eyes wild and glaring. M. Pihorel made him swallow instantly half a glass of olive oil, and applied the actual cautery to the wounds. The patient bore this, but speedily fainted, discharging the contents of the bowels and bladder. He continued torpid till half-past twelve, when he complained of cold, and vomited with subsequent relief. He then had a draught containing opium, Eau de Luce, a greatly reputed specific, and some sulphuric ether. Vomiting recurred at one P.M. He was ordered a sudorific ptisan, and a draught consisting of oil of sweet almonds and acetate of ammonia. The hand, which was slightly swelled and painful, was enveloped in a linseed powder poultice.

\* Philosophical Transactions, and republished in *Comparative Anatomy*, Vol. v. section viii. p. 139.

At three P. M. the patient was better. At four respiration was laborious, deglutition difficult, and the pulse between 96 and 100. At five there was pain at the shoulder, which was much increased at six, yet without swelling. At seven the countenance was depressed, the voice weak, and the patient was oppressed and anxious, while he entreated to be let blood. Ten leeches were applied to the anterior part of the neck.

At half-past seven respiration was stertorous. At eight the extremities were cold, the pulse imperceptible, and he could not swallow liquids. The mental faculties were entire; though the patient expressed great anxiety about his wife, whom he believed he should never live to see. A few minutes after, the lower extremities were suddenly extended; the patient attempted to take the sitting posture, but was unable; the head fell back; respiration became embarrassed with rattling; and the patient breathed his last at a quarter past eight. The whole duration of the illness, from the insertion of the poison to the fatal termination, was about eight hours and one-half.

The cellular tissue subjacent to the bite at the dorsal surface of the thumb was not infiltrated; but it was so around and over the second puncture.

All the veins at the back of the hand and their trunks examined as far as the arm-pit presented no alteration, and were in all respects similar to the same veins in the other arm. They were void of blood below the elbow; between the elbow and arm-pit they presented some disseminated clots. In the clavicular extremity of each axillary vein began a clot moulded in the interior cavity of the vessel, and which was continued to the insertion of the hepatic vein into the inferior cava, passing by the right auricle of the heart. This clot, every where homogeneous, presented only a slight degree of consistence, and not the smallest appearance of isolated fibrin. A large clot filled the left auricle of the heart, and the junction of the pulmonary veins; and in the centre of this clot was found a mass of yellowish fibrin, from fifteen to eighteen lines long, such as is usually found within venous clots.

The empty state of the two ventricles presented a strong contrast with the distension of the auricles, especially the right auricle.

The mucous membrane of the windpipe and *bronchi* was remarkably injected. A patch of inflamed membrane corresponded to the cricoid cartilage. The windpipe and bronchi were filled with a reddish frothy mucus. The lungs, though crepitating and sound, presented the first degree of bloody injection.

The other organs were normal.\*

\* Observations sur la Morsure d'un serpent a sonnettes, par M. Pihorel, D. M., &c. Journal de Physiologie Experimentale et Pathologique, par F. Magendie. Tome vii. Avril 1827. Paris, 1827. P. 97.



This was regarded by M. Pihorel and his associate M. Desmoulins as a clear instance in which the poison is transmitted by the veins; and the fatal result was by them ascribed to the operation of the poison which had been absorbed previous to the employment of the cautery. They conceived also that the cauterization, made fifteen or twenty minutes after the bite, had destroyed in the wound what remained of the poison.

Mr Webb gives, as illustrative examples, two instances of snake-bite,—one terminating favourably after the lapse of nearly one month; the other terminating fatally after the interval of about sixty hours.

In the first case, that of a feeble old man, who was bitten on the left outer ankle on the 28th September 1836, the wound was scarified and the cupping glass was applied, while ether and ammonia were given internally. On the 29th, blood was discharged from the mouth, nose, gums, and bladder; and much bloody serum issued from the wound. The hemorrhages ceased on the 2d of October. On the 17th, a bubo in each groin had appeared; but they gradually subsided under the use of poultices. He was rid of all bad symptoms on the 30th October; and a week after was quite well.

The other case was that of a hurkaru, aged 24, bitten the evening of the 13th July 1837, at a village eight miles distant from Balmir. Next day he was brought to the hospital. Blood oozed from the nose, mouth, and gums, and the urine contained much blood. This hemorrhage was much abated on the 15th, but he died on the morning of the 16th, about 9 A. M. No inspection is given.

In the observations on cholera, some facts are adduced to prove that this distemper is not new. This subject we need not resume in the pages of this Journal, in which the most ample proof that the distemper is not new, has been long ago adduced. The author ascribes the formation and prevalence of the disease to the presence of sulphuretted hydrogen, which, he says, is most abundant in districts where cholera prevails, as in Sirhind, Rajpootanah, Marwar, and the whole of Sindh. Indeed he maintains that both cholera and typhus proceed from the presence of this agent. In the hot weather of 1819, at Kurnaul, the artillerymen used frequently to lie down on the ground for coolness in the evening, and were often conscious of bad-smelling air issuing from the soil, which being breathed for a few minutes only, the men turned blue, and did not regain their colour unless speedily made to vomit and take largely of spirits. That was their treatment. The reporter did not know of any dying in this state. But the circumstances took place so often, that the men were wont, on perceiving the smell, to look about on the ground, and often found in the soil a

small hole, whence a jet of offensive air was issuing. According to Dr Kinloch Kirk, mephitic air is incessantly escaping from the rocks at Sukkur. This he seems to ascribe to the formation of sulphates forming under ground, while sulphuretted hydrogen and carbonic acid gases escape. By the same authority, the disease in Scinde is represented as peculiarly and most speedily fatal, destroying sometimes in the course of two hours. In Kurrachee, in July 1846, 1500 of the troops were carried off in nine days; of these, 890 were Europeans. During the hot weather of 1845, cholera visited Sukkur, and many other parts of Scinde, with great severity. In the cantonment of Bazar, from thirty to forty people died daily for some time.

In 1840, typhoid cholera prevailed in Calcutta, and the disease was then ascribed to malaria. The disease was not attended by violent symptoms. Vomiting was often entirely wanting. Not more than one in five patients had cramps; and, from several instances, it appears that only one watery stool took place. Another peculiarity was tedious convalescence, and low typhoid gastric fever ensuing, that is, a condition indicated by restlessness, tenderness of the epigastrium, deficient secretions, as scanty urine, scanty bile, with a dry glazed tongue and quick pulse.

The author mentions next the choleroïd colic of the Himalaya district. This disease, which is very common at Simla, is very fatal. There may be vomiting; but no alvine discharges take place. The belly is hard, tender, and swelled; the urine suppressed; the pulse gone; and the countenance anxious. The only chance of recovery is indicated by reaction taking place, alvine discharges being procured, and the secretion of urine being re-established.

The morbid appearances were observed chiefly in the abdominal viscera. The stomach was covered over its mucous surface with black, sooty-looking, thick, ropy mucus. The duodenum presented patches of ecchymosis. The mesentery was redder than natural; and a bright red line indicated its junction with the intestine. The ileum was dark-red from congestion, its mucous lining softened, but without effusion of lymph. The upper part was filled more or less with a dark coloured sooty-like fluid, without trace of bile, inodorous, and very like the black vomit of yellow fever. Little of this was found in the lower portion of the intestine. The cæcum outside had a puckered appearance; its inner mucous surface was eroded.

The bladder contained a small quantity of urine. The spleen was enlarged; the liver congested. The abdominal veins were distended, with dark coloured tarry, uncoagulated blood.

This form of disease originates in malaria. It is as common among those who live on the best grain, and use animal food, as

among those who live on bad grain and rice. The locality and its exhalations constitute the main agent; and this Mr Webb represents to be that from which the worst typhus cases come.

The treatment which the author confidently recommends consists in, *1st*, the hot bath for twenty minutes; *2d*, blood-letting afterwards according to its effects on the pulse; *3d*, sinapisms to the abdomen and the feet; *4th*, calomel and opium, followed by an enema of turpentine and castor oil, with asafetida, to be repeated every four or six hours until stools take place, when it is to be given up.

Four instances are given; one fatal. We shall afterwards find, that in this district, namely, the lower Himalaya, an intense form of fever called typhus is very prevalent. It is possible that this may be not typhus, but the continuous remittent, which we mentioned formerly as evidently the disease to which the Pali and Kutch and Kattywar fevers belong.

Choleroïd fever or the sweating sickness of India next is mentioned.

In May and June 1839 cholera was very prevalent in the neighbouring districts; and several cases occurred among the native troops in the cantonments. In June, July, and August cases occurred among the Europeans; and in August, September, and October ague and remittent fever, with cases of dysentery, became prevalent. The distemper called the sweating sickness then appeared in the end of June and in July, and again in the end of September and in October. It began with rigors and chilliness, followed by dull headach; heat of skin and dilated papils; thirst; burning sensation at the epigastrium; restlessness; and copious watery motions, smelling like the flesh of carnivorous animals slightly tainted. In many cases vomiting of a similar fluid took place; and the skin was bathed in perspiration. The pulse was weak and rapid; the breathing greatly oppressed, with epigastric anxiety; and extreme feebleness was a uniform symptom.

In the severest forms of the disease, all uneasiness soon ceased, excepting that arising from thirst and pectoral oppression; coma ensued; and death took place within ten hours in some cases after the period of attack.

In more favourable cases the pulse became full and slow; precordial oppression abated and disappeared; the bowels acted; urine was discharged; and the patient slept.

“By far the most remarkable morbid appearance,” says Dr J. Murray, “is the great alteration in the blood from its healthy state. Its vital properties are diminished; its power of coagulation is weakened; the coagulum is less firm; and colouring matter is partially diffused through the serum. The blood usually has a very dark colour; though in some cases it was known to be fluid a few hours

before collapse supervened. The serum had a muddy appearance and a peculiar smell. The blood was probably less stimulant to the heart than in its natural state."

"In this disease are found all the symptoms produced by the retention of those parts of the circulating fluid, usually removed by respiration; as lividity of the countenance, depressed heart's action, and congestion in the large veins; and we find also those caused by suppression of the urinary and biliary secretions, as vomiting, coma, and serous effusion within the cranium. In addition to this, there is the periodic return of the symptoms with excessive perspiration, and the watery stools having a rank nauseous smell, which is only to be accounted for by the existence of a morbid condition of the blood, or the admixture of some deleterious substance with it."

This disease is manifestly the *tritæophya elodes* of the ancients and Sauvages. It cannot be regarded as a typhous disorder.

It is the result of telluric miasma of a particular form and a very concentrated degree of force, probably occasionally the offspring of the combined influence of telluric exhalations, and some atmospheric derangement. The description given by Sauvages from Boyer, accords closely with that given in the present work.

It was stated in the beginning of this article, that various diseases common in cold and temperate latitudes might be supposed to be either of unfrequent occurrence, or quite unknown in India; and that, nevertheless, the physical circumstances of India, and the habits of its different tribes of occupants, are so peculiar, that nothing but direct evidence from the country itself ought to satisfy the unbiassed inquirer. It appears that it has been generally believed, that diseases of the heart and blood-vessels were either unknown or very rare among the natives of India; and that, whatever instances of disease of these organs took place, were chiefly, if not exclusively, in the persons of Europeans. In 1847 Mr E. Crisp published a work on the diseases of arteries, in which it is stated, on the authority of Mr Webber, that aneurism is either unknown or extremely rare among the natives, both Parsees and Hindoos, and that organic diseases of the heart are equally rare and little known. According to one authority, indeed, the rubric or title of heart disease or aneurism does not appear in the government reports. The evidence is allowed to be that of a visitor, and consequently might have been obtained under circumstances unfavourable for precision.

It appears, however, from some observations in a review of the first edition of Mr Webb's work, in the Indian Journal of Medical and Physical Science for 1845, that the same idea of the small prevalence of diseases of the heart and arteries among natives in India is entertained at Calcutta. From the fact that most of the specimens of altered structure in the tissues of the circulating system were

taken from the bodies of Europeans, it was naturally proposed as a question, whether the natives of India are exempt from aneurismal tumours, and in what degree they are so. With the view of furnishing means of answering this question, Mr Webb gives, in the present edition, much pointed information. It thence results that disease of the heart and arteries is an affection not uncommon in the natives of India.

In the first place, the author adduces examples of *endocarditis*, *aortitis*, disease of the valves, and atheromatous degeneration of the aorta in natives of India. These specimens are in number twenty-nine. They may be classed under three heads; first, instances of inflammatory diseases of the cardiac tissues; secondly, examples of chronic diseases of the cardiac tissues; and, thirdly, instances of acute diseases either of the heart or contiguous organs taking place in persons labouring under chronic disease or diseases of the cardiac tissues.

Among the second class is one case of that rather rare disease, —aneurism in the muscular structure of the heart. There appeared to be no proper right ventricle; but the pulmonary *atrium* was very much dilated, and at its most dilated portion it had burst. An aneurismal enlargement about the size of a small fowl's egg had taken place in the apex of the left ventricle, and had encroached on the capacity of the right ventricle.

In the third class may be mentioned an instance of small-pox pustules, being deposited outside the heart, within it, and upon the aorta and pulmonary artery. If these be really small-pox pustules, the case is singular. But it is so easy to mistake deposits of lymph, or of tubercular matter, for pustules, that it must not be regarded as a proof of excessive scrupulousness if we take the liberty of expressing doubts on the point.

Of aneurism of the arch of the aorta the instances are five;—abdominal aortic aneurism, one; carotid aneurism, two cases; aneurism of the external iliac, one case; aneurism of the femoral artery, one case; aneurism by anastomosis, one case.

Displacement of the heart in consequence of empyema is a common case in this country. One instance, in a native, combined with *endo-pericarditis*, inflammation of the aorta, atrophy of the left lung, and tuberculosis of the lung, is mentioned.

It is remarkable that many of these lesions occurred in persons who had been exposed either some time previously or very recently to the effects of telluric miasms, and, in some instances, in those who had been confined in jail.

These facts are quite sufficient to disprove the idea that diseases of the heart and arteries are rare among the natives of India.

These diseases are still more frequent, however, in Europeans in India. These may be distinguished as those occurring among

natives into three classes. There are mentioned eight instances of aneurism of the thoracic part of the aorta, one of the abdominal aorta, one of the coronary arteries, one of the right carotid, which proved fatal by bursting externally, and one of the external iliac artery.

In several of the cases of endocarditis, the substance of the heart was more or less dark-coloured and injected, and a little softened.

In several of these cases, the chronic disorder of the heart had subsisted for some time; and, though it could not alone have occasioned death, yet it contributed to the fatal event. The individuals were attacked by cholera, fever, or some similar acute disease. The symptoms were then complicated with symptoms of disorder of the heart; and death took place as the result of the combined effects of both. Thus, while cholera has evidently been the direct means of destroying several of these patients, Kurnaul fever appears to have been also an auxiliary. In one instance, a patient with dysentery, a young man aged 23, is treated by blood-letting with other remedies. The vein of the arm becomes inflamed; and the patient is destroyed by the effects of extensive *phlebitis*.

The author then enters on some general observations on the nature and causes of diseased states of the heart and aorta, and other arteries. In these observations we find, first, some notice of the observations of ancient physicians, especially Galen, on the occurrences and causes of diseases of the heart. All that we observe on this subject is, that by far the most intelligent information is given in a small but interesting treatise, in the present time too much overlooked. We refer to the learned and sensible inquiry on *Carditis* by Dr John Ford Davis, published at Bath in 1808.\*

In these observations, Mr Webb is a great patron of the omnipotence of inflammation in inducing morbid changes. He is convinced, he tells us, that nearly all the specimens of structural changes described by him as occurring in the heart itself and great vessels, whether derived from Europeans or from natives of India, have had their origin in inflammation. In many of them, the phlogistic action commenced in the pericardium; and not merely by consent of parts, but by contiguity of parts, of the two serous membranes, endocardium, and pericardium in the auricles, the action has passed from the outside to the inside of the heart. Effusions of lymph have then taken place; sometimes small, such as granulations and vegetations, about the valves; sometimes spread out as false membranes, lining the whole of a cavity, or agglutinating the valves; and sometimes accumulated in masses

\* An Inquiry into the Symptoms and Treatment of *Carditis* or Inflammation of the Heart; illustrated by Cases and Dissections. By John Ford Davis, M.D. &c. Bath, 1808. 12mo. See pages 141—144.

as polypi, which, remaining long, have become completely organized, and even lined with a membrane, continuous with the endocardiac lining of the heart.

To those accustomed to observe the phenomena and reason on the origin of cardiac diseases, these views, excepting the statement last made, have been long familiar. All pathologists almost agree in ascribing the changes mentioned to a form of inflammation; but that inflammation, especially where it affects the valves, they regard as most usually of a chronic character, and therefore approaching to a form of perverted nutrition. It must be remembered that, on this point, it is difficult to obtain more than probable and conjectural evidence. Positive and demonstrative evidence of the principle it is impossible to procure. Now all that can be deduced from the facts, appears to us to be this,—that these granulations and vegetations are most usually the result of some form of chronic inflammation, or at least a state of perverted nutrition; and that occasionally inflammation may be superadded to the process, by which they are produced.

The concluding statement, that the lymph is sometimes accumulated in masses like polypi, shows that Mr Webb is rather decided as to the organization of polypous clots; and probably he is, in some respects, right. It is well known, that a considerable revolution has taken place in the opinions of pathologists on this point, and on the nature of those masses of fibrine often found adherent to the *columnæ carneæ*, to the *musculi pectinati*, and other parts of the internal surface of the heart. At one time it was imagined that *polypus cordis* was a frequent disease during life, and a frequent cause of death. Then, as morbid anatomy was cultivated, and its indications were more thoroughly understood, it was boldly maintained, that such clots and formations were incompatible with the action of the heart at all, and with the continuance of life in any form; and it was confidently taught, that the polypous coagula were formed in the moment of death, and after the heart had ceased to act. For a long time this was the established doctrine. Various observers, however, struck with the firm adhesion of these fibrinous clots, and, above all, remarking upon their adherent surfaces and points minute spots of blood exactly similar to those seen in the lymph effused between corresponding surfaces of serous membranes, began to think that these must indicate organization, and that the fibrinous clots must have been formed in and in some way separated from the blood, then attached themselves to contiguous surfaces, and thus presented the appearances of incipient organization.

The views on these bodies given by Corvisart appeared so rational, and tended to rectify so well the two extremes in opinion

now noticed, that many acquiesced in their correctness. Corvisart distinguished polypiform concretions into three sorts.

1. Those which are of longer or shorter standing, and which may be known by their pale fleshy colour, their density ; their fibrous or fibrinous organization ; and, finally, by the force with which they adhere to some part of the cavities of the heart.

2. The polypous-like concretions formed in the latter days of life. These are distinguished by their yellow lymph-like colour, sometimes reddish ; by the smallest possible degree of fibrinous disposition, yet essentially coagulated ; and by adhesions extremely weak, and showing rather slight application than attachment.

2. A sort of *magma* or coagulum, similar to over-done currant jelly, which breaks down under the finger, adheres to nothing, which has no fibrous or any analogous appearance, which is formed after death by rest and cooling, by spontaneous elective attractions, variable according to age, temperament, the duration of the disease, the peculiar and degenerated crisis of the blood.

This distinction he acknowledges not to be logical, and as presenting rather three degrees or states of the same condition than well-marked species. He thinks it nevertheless exact in observation and useful in practice ; and he then details two instances illustrating the existence of polypi of the first species.

Corvisart subsequently adds, that, in several circumstances, he had shown the pupils polypiform concretions of a yellowish-white colour, of fibrinous structure, so consistent, so tough, so intimately adherent to the internal fibres of the heart, that he could not refuse to admit as a fact established by experience, the formation of these concretions a long time previous to the death of the individuals, in whose persons they were observed ; and concretions the existence of which he had sometimes announced before opening the body, reasoning from the nature of the symptoms peculiar to the disease of these patients.

"Assuredly," he continues, "from inspecting this substance, from its consistence, the firmness of its attachment, and its interlacements, it is impossible to refrain from believing, that it was formed long previously. Of this, indeed, I never had any doubt."

Such bodies, he further thought, might, by their presence, give rise to the symptoms of disease of the heart ; but such symptoms appeared not to him sufficiently well-marked to guide the practitioner in establishing a just diagnosis. Generally the presence of polypiform concretions in the heart gives rise only to symptoms which are temporary and irregular in their return. When they are floating within the cavities of the organ, they cause marked derangement in the phenomena of the circulation, only in so far as the current of blood carries them, either to the auriculo-ventricular



orifice, or to the mouth of the large vessels. Blocking up, then, partly or entirely the diameter of these organs, they induce palpitations, fainting fits, which are most commonly transitory, because they subside as soon as these concretions quit the orifices which they obstructed, and float as formerly more or less freely, according to the extent of their attachment to the walls of the heart, in the interior of the cavity in which they are contained.

But when any of these polypiform bodies are attached to certain points, then they cause constant symptoms. This takes place especially, he had several times observed, when any one of these concretions is entwined with the valvular cords and the valves of the heart, to which they strongly adhere. In these circumstances they cannot fail to prevent the freedom of the functions of these membranous folds, by rendering them almost immovable, by the unnatural adhesions which they establish between them and the walls of the heart, to which these concretions partly adhere.

Corvisart further regarded the accumulation of blood and its prolonged stay within the cavities of the heart, and a peculiar disposition of this liquid opposed to the serous state in which it often is observed, as the principal causes of the formation of polypi. This accumulation, again and prolonged stay of the blood within the heart, are often the effect of organic disease of the organ or its appendages,\*

These views were, on the whole, adopted by Laennec, who further confirmed the conclusion that the blood may, by being detained within the cavities an undue length of time, become coagulated and attached to the walls of the heart. †

We have seen that, in general, Corvisart ascribed to the presence and operation of organic disease the tendency to accumulation of blood within the chambers of the heart. Though the correctness of this, as a general inference, cannot be denied, by some it was maintained that these polypiform concretions, adherent to the walls of the heart, took place in certain instances where no evidence of old organic disease, even after death, could be detected. Though the exact accuracy of this statement may well be questioned, yet, with many it has been admitted; and other causes have consequently been sought for, to explain the occurrence of polypiform concretions, and their firm attachment to the internal surface of the heart. Some of these are probably to be found in the state of the blood, especially in diseases of the heart and of the kidneys. And others perhaps are connected with the inflammatory state in veins, in which there is no doubt that the occurrence of inflammation de-

\* *Essai sur les Maladies et les Lésions Organiques du Cœur et des Gros Vaisseaux*; par J. N. Corvisart. A Paris, 1818. Troisième edit. Corollaires Art. viii., p. 474.

† *Traité de l'Auscultation Médiate*, &c.

termines the coagulation of the blood, and interruption to its motion. It was, at all events, obvious that, supposing the fact established, that blood might become coagulated and clots formed, and become attached to certain points of the inner surface of the heart, the question might naturally occur, why these clots should then attach themselves to these surfaces, and what was there in these surfaces at that time to determine this attachment? The answer was, that these surfaces must have been, and actually were, in the orgasm of inflammation. The authority by whom this idea has been mostly favoured is Bouillaud. He does not, indeed, at all times and in all places, express himself with perfect consistency or identity. But it is nevertheless easy to see that this is the ultimate solution which he offers of the problem. Like Corvisart, he distinguishes three sorts of concretions. The first are the amorphous, recent, quite similar to ordinary clots of blood. The second are organized, and present different characters, according to the stages of their evolution. In the first stage they are white, analogous to lymph, elastic, adherent to the walls of the cardiac chambers, and especially to the *columnæ carneæ*, and the *chordæ tendineæ*, round which they are entwined. The third form is when these concretions are in a more advanced stage of organization, and then they adhere by true cellular tissue to the parts upon which they are formed; thus engrafted on living parts, they are penetrated by vessels, and become hardened; and they then resemble closely certain fibrous polypi, tumours, or fungous vegetations.

Subsequently his language is the following:—"As to the chemical and vital causes of blood-concretions, this species of crystallization of the blood, the principal are idiopathic or consecutive inflammation of the internal membrane of the heart, and the introduction of different foreign substances into the circulating current, for instance, purulent matter. My intention is not to insist long on the part which inflammation performs in the development of bloody concretions, considering that I have explained elsewhere (at pages 175, 176, 181) the hypotheses which may be formed on this point. I merely add, that all manifest inflammations, accompanied with violent febrile reaction, and in which the blood drawn presents a good white firm elastic, resisting buffy coat, constitute a genuine predisposition to certain fibrinous concretions in the heart, which, as we have seen, present then a great resemblance to the inflammatory crust. Consult also the cases which we have recorded in this work, and you will see that the most of the instances in which fibrinous concretions proceeded not from simple embarrassment in the circulation, they corresponded either with idiopathic inflammation of the heart, or with inflammation in another organ, which reacted forcibly upon the heart,

as upon the whole of the circulating system, and on the mass of blood.” \*

This explanation Mr Webb appears to espouse, and rather strenuously to defend. The formation of fibrinous adherent clots appears to him to be not only a recognised and established fact in pathology, but he thinks that the circumstances under which it takes place, are chiefly those which indicate previous inflammation of the lining membrane. It appears that many of his brethren in India are skeptical as to the existence of organized polypi in the formation of membranes, that is, as we understand, false membranes in the heart at all; and at this we do not wonder. It is a doctrine which is embarrassed with not a few objections; and facts show, that it is altogether impossible to admit, that all the adherent clots found in the chambers of the heart after death had existed during life, and are organized. We allow, nevertheless, as all, we believe, must do, that instances of this kind of phlogistic and organized clots take place; yet they are greatly more rare than is probably imagined; and we are rather disposed, from what we have seen of instances of this sort of production, to ascribe them with Bouillaud, either to inflammation of the veins, or to some degree and form of inflammation of the lining membrane of the heart.

Of this occurrence, Mr Webb gives, at page 69, what may be probably regarded as a good example. The essential circumstances are the following. At the root of the superior *cava*, a membranous layer is seen adhering inside, and another at the root of the inferior *cava*. The fibrin has accumulated as a clot in the auriculo-ventricular septum. It invests as a thin layer of false membrane the upper and the under surfaces of the tricuspid valve, and is prolonged in the *columnæ carneæ*, inextricably involving the cords by thin cobweb-like films. In the pulmonary artery, about half an inch above the semilunar valves, is a sort of additional valve, formed of fibrine attached below, loose above. Another similar formation is seen in the arch of the aorta. Fibrinous exudations are attached to various parts of the left auricular chamber, and a thin lamella extends through the auriculo-ventricular opening, covering each side of the mitral valve, and coating the columns and chords. The substance of the heart resembles boiled cow's udder,—a change which the author believes it often undergoes in acute inflammation. The whole external surface of the pericardium was covered by lymph.

The patient had, besides, suppurating tubercles in the lungs and pleuro-pneumonia.

It appears, further, that these inflammatory attacks, whether

\* *Traité Clinique sur les Maladies du Cœur*, par J. Bouillaud, Prof. de Clinique Médicale, &c. Tome ii. onde. Paris, 1825, Appendice, p. 612.

affecting the pericardium or the endocardium, or, as is most usual, both membranes, seldom take place alone. Often, indeed, most frequently, they are associated with or the consequence of fever, of hepatic abscess, of inflammation of the lungs, of inflammation of the brain, of acute dysentery, of *gastritis*, as was actually the case in the preparation here specified.

Some observations on *arteritis* and its effects, arctation, and obliteration of arteries, follow. But as they are principally taken from, or illustrative of the observations of Tiedemann, whose work was examined in detail in the sixty-sixth volume of this Journal,\* and as the subject of arctation and obliteration was considered in the fifty-sixth volume, we need not dwell on them. We merely observe, that the passage quoted in page 73, as from the London Medico-Chirurgical Review, is from this Journal.†

The author quotes from a paper by Dr Wyse, in the eighth volume of the Transactions of the Calcutta Society, an example of that rare lesion, obliteration of the aorta at the definite point. The patient was a strong middle-aged native of India, who, while walking, suddenly fell down and expired. The aorta was found quite obliterated, and reduced to a cord like the *ductus arteriosus*, which indeed marked the place of constriction. It then became again of its natural size, receiving large arteries. The aorta was ruptured immediately above the valves (p. 75). This case corresponds, in all its leading characters, with those already recorded. The arctation the author regards as congenital; and in this there can be no doubt that he is right, as was shown several years ago by Dr Craigie.‡

One great cause of the frequency of these disorders of the pericardium, the endocardium, and the heart, the author considers to be found in articular rheumatism, which both in the acute and chronic forms, is stated to be more universally diffused over India than any other disease whatever (p. 77). It is seen in all the three presidencies, and its prevalence is ascribed to the same climatic causes as those which give rise to ague and remittent fever. It is represented to be a very intractable and lingering complaint, often ending in disorganization of the joints, in which it is seated. It spares no age. The young, the adult, and the old, are all equally subject to it. Children between ten and fifteen years of age are often brought to the Jubbulpore Dispensary, labouring under rheumatism; but a palsy-producing form of the disease, which is stated to be very common in Dyah Burroghur, a Pergunnah, in the district of Allahabad, is very rare at Jubbulpore. These facts show that even warmth may not prevent the approach of rheumatism.

\* Edin. Med. and Surgical Journal, Vol. lxvi, pp. 111 and 413. Edin. 1846.

† Ibid. p. 419.

‡ Ibid. Vol. lvi, p. 427. Edin. 1841.

There is recorded, at p. 83, a melancholy case, in which *carditis*, *endocarditis*, *arteritis*, and inflammation of the veins, were all combined in a stout and bold, but rather dissipated, European of 40 years. The attack of inflammation of the veins was caused apparently by his getting one of the apprentices to tear open forcibly the wound in the vein of the arm, in order to lose some more blood, which he maintained was necessary to afford adequate relief. The fourth day after, he was attacked with severe rigors. The symptoms of intense phlebitis followed, and, on the eighth day, the patient expired. Will Mr Webb allow us to remind him, that the affection of the liver mentioned, as observed in this man's case, is *cirrhosis*, or *kirrrosis* rather, not *schirrosis*?

On the employment of the term Atrophy to designate a particular state of the heart, Mr Webb is disposed to criticise a little censoriously. After quoting from this Journal the description of atrophy given by Professor Tiedemann, and in which we believe the professor of Heidelberg is not solitary, the author adds, that in the singular specimen of this disease which is preserved in the Calcutta Museum, the cavities are dilated, and the heart resembles a bladder-like pouch or sac covered with fat. A strange thing he thinks it to call this production of fat, impaired nutrition depending on diminished supply of blood. The explanation of Senac, who represents the fat to compress the arteries and impair their action, and also those of the nerves, he thinks the most eligible of the two.

Now it must be here observed that the term atrophy is employed in two different senses; and we are not sure whether it be not applied either to two different lesions or to different stages of the same lesion. First, it is impossible to doubt that one form of atrophy is the effect of arctation of the coronary arteries, or complete closure of one of them, or of its branches; in short, of a state in which these vessels no longer perform properly their functions of canals for transmitting the blood to the muscular fibres of the heart. It is unnecessary to quote our own description, which may be found at pages 443 and 444 of volume sixty-ninth. But we feel it requisite to say, that the description must be perused again, and perused along with what we have now to say.

Secondly, The muscular fibres of the heart are liable to the fatty transformation, or *steatosis*, and by a good glass and the microscope, particles of fat or oil may then be seen deposited in the place of the muscular fibres, and within their cylinders. This is another form of disease, to which the name of atrophy is often applied. Whether it be the advanced stage of that already noticed, or a different form of the affection, we feel not competent to say. But this is certain, that such deposition and infiltration of fat is a most serious form of impaired nutrition or perverted nutrition, no matter which;—for it is idle to dispute about words. We have formerly

said that all misplaced depositions of fat are indications of atrophy, and in this light must be viewed this infiltration of fat where no fat should be, and where muscular fibre should be found. On the effects and indications of fatty deposit in the arteries, the kidney, the liver, the lungs, and the heart, we shall do no more than refer Mr Webb, and our readers in general, to the observations by Dr Inman in volume sixty-sixth, page 40, and to what has been said in volume sixty-ninth, page 449. The paper of Mr Hallett, also, in the present number, may be consulted with advantage.\*

Softening of the heart, as a lesion, the effect of inflammation, is next noticed. That softening is distinguished by lacerability of fibres, and is the effect not only of inflammation but of fever, and probably various states of the system in which the blood is more or less diseased.

The author concludes this division of his work by the inference, for which all his readers are doubtless prepared, that diseases of the heart and circulating organs are very common in India, and that these diseases have, for the most part, an inflammatory origin. He adds, that, from this cause only, great numbers of soldiers are annually placed on the invalid list; and he goes so far as to say that the newly-arrived cadets and soldiers, when suffering from what is called seasoning fever, often exhibit what he believes to be *endocarditis* in an acute form.

It is certain that various lesions of the heart, and especially softening, are the effect of the telluric fevers in India, as at Kurnaul, and in various parts of Bengal. In that tedious and unmanageable affection called *berri-berri*, softening of the heart appears to be the cause of death. In all these instances, a poison has been circulating with the blood and acting on various organs; and it seems most natural to think that this poison, namely, the telluric, or perhaps a telluric aerial exhalation, is the main cause of all these evils.

The second division is devoted to the lesions in the organs of respiration.

It has been usually supposed, by the inhabitants of the cool and temperate countries of Europe, and more or less inculcated by physicians themselves, that diseases of the lungs and air-passages were much less prevalent and less severe in warm and equinoctial countries than in climates less exposed to high degrees of solar heat. This idea has evidently been adopted upon some-

\* Elements of Pathological Anatomy, by David Craigie, M. D., &c., 2d edition. Edinburgh, 1848. Book III. Chapter I. Section II. Steatoria, pp. 407 and 1062.

thing like the principle of antithesis. It was known as a matter of observation, that diseases of the lungs and air-passages were frequent and severe in cold and temperate countries, and it was therefore inferred that they ought to be rare and mild where the solar heat is greater and more constant. Upon a little reflection, nevertheless, it seems singular that this notion could have ever gained any firm hold of the mind. It is at variance with the testimony of all writers, ancient and modern; and it is contradicted by daily experience in many parts of the globe. The testimony of Aretaeus, Celsus, Pliny the elder, and Coelius Aurelianus, among medical writers, and the evidence of various miscellaneous authors, show that diseases of the lungs, and, above all, phthisis, were very frequent disorders in all the warm countries extending round the coasts of the Mediterranean. From the descriptions left us by Coelius Aurelianus, the great eclectic of antiquity, it is manifest that pneumonia, pleurisy, empyema, bronchitis, and phthisis, were very frequent affections in ancient times. This physician, who is believed to have lived about 180 years after the commencement of the Christian era, in the reigns of Trajan and Adrian, was a native of Africa, but had been in several of the towns of Greece, and eventually lived at Rome; and as he shows perfect and accurate knowledge of the pathological doctrines and therapeutic methods of his predecessors, it is reasonable to conclude that the diseases most usually requiring treatment, would occupy the largest amount of his attention. The diseases above specified, accordingly, with various febrile distempers, affections of the brain, and dysenteric and other intestinal disorders, occupy a conspicuous portion in his treatise.

Among the ancient of the modern authorities, Prosper Alpinus furnishes evidence of the same tendency. This author,—who was a Venetian, and professor of medicine at Padua, visited Cairo in the year 1581, in January, and remained there till 1584, collecting information on the habits and diseases of the Egyptians. Among other matters of interest, he states, that, among the diseases most prevalent among the Egyptians, next to plague, fevers, elephantiasis, gout, and ophthalmic disorders, are to be placed affections of the lungs, malignant catarrhs, and phthisis.

Independent of this, however, it is well known, that, both in the countries all round the Mediterranean and in the West India islands, diseases of the chest are not only common, but severe and rapid in progress; and the experience of the French as to many parts of the north of Africa, our own as to Syria and other places, show clearly that countries in which the temperature is high, are so far from being exempt from pulmonary disorders, that these appear to be there quite as prevalent as in cold and temperate latitudes, and assuredly more intense in symptoms and more rapid in progress.

We are, therefore, not surprised at seeing these views corroborated by the facts contained in the present volume, and finding that India only adds one more to the list of countries situate in and near equinoctial latitudes, in which pulmonary disorders prevail extensively and severely.

The diseases affecting the respiratory organs in India are quite the same as those which prevail in countries more remote from the solar road. They take place both in natives and in Europeans. They consist in bronchitis and bronchiectasis, or dilatation and hypertrophy of the air-tubes, pleurisy and its consequence empyema, pneumonia of various kinds, hæmoptysis and pulmonary apoplexy, tuberculation and its effects, vomica, and vomical empyema. On these points, the author communicates much multifarious information; but the arrangement of his materials cannot be commended.

The author commences by giving a tolerably flat denial to the representations and statements usually made, that diseases of the lungs are not frequent in India. He maintains, and adduces various facts to show, that pulmonary diseases, and especially tubercular diseases, are quite as prevalent among the natives of India as they can possibly be in more temperate latitudes, and that these disorders yield in frequency only to fever, cholera, dysentery, and affections of the spleen.

It appears that chronic catarrh, or bronchorrhoea, is a disease not uncommon in India, and that it affects two classes; one children, chiefly young girls, from five years up to thirteen; and the other old residents of sixty years, in whom it alternates in diarrhoea. In the first division of subjects he states that it is not uncommon for the tracheo-bronchial membrane to furnish one or two pints of muco-purulent sputa in the course of twenty-four hours. When it takes place in those in whom diarrhoea has been present, it appears to be the result of metastasis, as the intestinal discharge has been suddenly checked; and it then induces symptoms of oppressed breathing, so urgent as to threaten death from suffocation. In the other case it is liable to be mistaken for phthisis. But from this it is to be distinguished by its shifting character, affecting sometimes one bronchial tube, sometimes another, its being less permanent than the expectoration is in cases of phthisis, and by the previous history of the case.

The bronchial membrane in this division offers, after death, only evidence of relaxation, slight softening, and swelling, scarcely appreciable when immersed in spirit, or not more than would be with the intestinal mucous membrane after diarrhoea or cholera. The same state he represents to be observed in whooping-cough and croup, which are very common.

This accords little with what is observed in this country in



fatal cases of hooping-cough; for in these the bronchial membrane is often found extensively reddened and villous, as if it had been the seat of inflammation, which doubtless had been the case.

In Upper India, that is, in the lower Himalaya district, and in Simla, diseases of the lungs are common. In the cold season pneumonia and broncho-pneumonia prevail; and among the feeble and the old the disease assumes the asthenic type. In cases in which the disease assumed this form, if we understand the author aright, the patients had been previously labouring under some other disease, or, at all events, some other disease was present, as we are informed, namely, diarrhoea, dysentery, or some form of sloughing sore, with great debility. Careful inspection then showed that there was dyspnoea, and other marks of disordered breathing; while the breath exhaled a fetid smell, though not amounting to that produced by gangrene. On inspection after death various parts of the lungs were found consolidated, of a dark or brown colour; while, instead of the third stage, or gray softening of the sthenic character, were cavities containing mud-coloured fetid serum. These changes were most frequent in the posterior parts of the lung. Pleurisy arising in such subjects is attended with effusion of lymph, less plastic than in the sthenic form of the disease; and adhesions are softer, and more easily broken.

This form of pneumonic disease Dr Goodeve generally found in the badly-fed and badly-clothed. The existence of pulmonary diseases in such numbers, and in such subjects, he thinks, explains the high mortality which constantly attends dysentery among the natives.

A considerable number of years ago we took occasion to advert to the large mortality in the Indian jails, and the circumstances under which that mortality took place.\* The tract or work which formed the basis of these observations gave, we were informed, considerable offence to various persons in India; and the author of that tract, Dr Malcolmson, thought it incumbent on him to publish in this journal a sort of vindication or defence of his statements and representations.† The offence which was taken, was, that it was imagined that, by directing attention to the very large mortality in Indian jails, a censure was thereby meant to be pronounced on the medical officers, who thought they were accused of either killing the prisoners, or allowing them to die. It is extremely difficult, when human beings die in considerable numbers, especially in confinement, to obviate and evade the inference that some person or persons are to be blamed, and are chargeable

\* *Edinburgh Med. and Surg. Journal*, Vol. xlix. April 1838.

† *Ibid.*, Vol. li., p. 111. January 1839.

with the guilt of at least allowing these persons to die. In almost all cases, however, the medical attendants have little or nothing to do with the sickness and mortality ; and it would be quite as rational, to accuse them of wholesale homicide, because the entire building and all its inhabitants were instantaneously destroyed by lightning from heaven, or swallowed up in an earthquake. There is in all imprisonment a tendency, more or less powerful and efficient, to the production of disease. Not only are the prisoners confined and deprived of liberty, and exercise, and fresh air, and everything which liberty implies, but they are crowded, often in great numbers, in very small, confined, and ill-aired apartments, sometimes in the winter chilled with cold, and in the summer smothered with excessive heat ; and when disease attacks them, they are not only too weak to withstand its assaults, but the disease or diseases attack in an insidious form, which prevents them from being early recognised, while it renders all treatment more or less inefficient and abortive. All the prisons in this country have been, and still are, more or less remarkable for their disease-producing qualities. Dr Baly and our English friends say, that in the Scottish prisons, this is caused by the limited and scanty allowance of food. This may possibly have some influence. It must not, however, be forgotten that the dietary of all the prisons in every part of the kingdom, not excepting the most parsimonious of the Scottish prisons, is greatly superior to that which the agricultural and day-labourer can procure for himself, his wife, and his family, in any part of England, or Scotland either.

The truth is, that, as we have already said, there is in all confinement a tendency more or less powerful to produce disease. It is the cold and confinement during winter, and the hot and suffocating air of the prison in warm weather, that most usually renders the system disposed to disease, and not unfrequently actually creates disease. It is also undoubted, that confinement induces in those accustomed to freedom a depressing effect and a spiritless state of mind, which is exceedingly favourable to the production of disease. We have seen sent from prisons, otherwise regulated with great care, cases of chronic bronchitis and phthisis, manifestly induced during the short period of confinement.

This cause of pulmonary disease prevails largely in India. Mr W. A. Green met in the Midnapore jail with an unusual amount of pulmonary disease, principally in the form of pneumonia, with some cases of phthisis and bronchitis or asthma. Midnapore jail is an old fort, enclosed all round with a high entire wall ; and is therefore not provided with adequate means for securing supplies of fresh air and free currents through the different apartments. The inclosed area is further subdivided into separate closes or small areas by high walls. The ground on which the building stands

is rock. When it is said that the solar heat, thus radiated and reflected from a dry, rocky surface, and a great number of enclosing and containing walls, while no adequate channels are left for the admission of air, or for cooling that which is contained within the fort, it will be at once perceived that the building is uncommonly well calculated for producing among its inhabitants those diseases which depend on excessively high temperature, want of ventilation, and the enfeebled state of the system, produced by crowding numbers of human beings within limits too small for the purposes of proper exercise and healthy ventilation.

The soil, or ground on which the fort stands, is further believed to be favourable to the production of cough, and other indications and effects of pulmonary disease. Mr Green finds, at all events, that, in the rocky parts of the Zillah, cough is prevalent. The soil of the high ground, on which Midnapore stands, is clay ironstone. The atmosphere is comparatively dry, in consequence, Mr Green infers, of its traversing and resting on a heated metallic ground; and he thinks it probable that the exposure of the prisoners, in their daily out-of-door labour, to an atmosphere loaded with heat from an iron-bound soil, and permeating the delicate vessels and air-tubes of the lungs, must be a fertile source of mischief, aided by the additional evil, that they breathe the confined air of the jail during the night. In short, Mr Green infers that this heated air may produce, upon the texture of the lungs, effects similar to those resulting from inhalation of mineral and other minutely divided dust floating in the manufacturing atmosphere of Sheffield, as described by Dr Holland. Mr Green further regards the climate of Midnapore as inimical to lungs with tendency to disease. For a great part of the two years between May 1843 and May 1845, considerable numbers of the prisoners were employed in excavating a large tank out of the rock. During the work, their exposure to heat and dust was very great; and during the same period, the hospital returns show a decided increase in the prevalence of pulmonary diseases.

Neither can it be argued that these persons were ill or in an unhealthy state previous to confinement. Many of the men, he states, were previously strong, capable of earning their livelihood, and not subject to cough or any other symptom of pulmonary disease. On the contrary, Mr Green found, that after they had been inhabiting the jail, and had been working on the roads for a few weeks or months, they became the subjects of attacks of pulmonary inflammation, which recurred several times, terminating sometimes in recovery, sometimes in death, or incurable disease of the lungs.

In cases of undoubted phthisis among nine prisoners labouring

under this disease, Mr Green found that in one, pulmonary symptoms were developed on the fifth time of admission, after having been a prisoner for nearly twelve months; in a second, the prisoner worked on the road for two years before complaint; in a third, four months of imprisonment elapsed before symptoms appeared; in a fourth case, the prisoner was well during the first year of confinement, but frequently ill with fever during the three next years, at the end of which time, consumption became manifest; in a fifth case, pulmonary disease appeared at the end of the second year of imprisonment; in a sixth, the prisoner worked in apparent health for two years; in a seventh, the prisoner continued well in jail, performing daily labour for six months, and tubercular disease was not suspected till the eighteenth month of imprisonment; in an eighth, the existence of the disease was first suspected at the end of three years and a-half, the prisoner in the meanwhile having been subject to attacks of fever, in other words, attacks of inflammation of the lungs; and, in the ninth case, the prisoner, who stated he was well and strong at home, was frequently in hospital with fever during eight months, and then, in the eighth month of imprisonment, symptoms of disease of the lungs became evident.

It further appears, that, from the beginning of February 1844, to the end of May 1845, a period of sixteen months, the total number of prisoners in Midnapore jail was 14,313 persons. Of these, during that time, there were 2339 admissions to hospital. The admissions for pneumonia were 175; for bronchitis, 10; and for phthisis, 14. Of these, twelve cases of pneumonia, and seven cases of phthisis, terminated fatally. In 160 cases of pneumonia, recovery took place. The average duration of these cases was eighteen days. The average duration in the fatal cases was twenty-eight days. The average duration of the disease, in the fatal cases of pulmonary consumption, was 112 days. The ages of the fatal cases of pneumonia were, between 10 and 20 years, one; between 20 and 30, two; between 30 and 40, eight; and, between 40 and 50, one. The ages of the fatal cases of phthisis were, between 20 and 30, three; between 40 and 50, three; and, between 60 and 70, one.

In the persons sent to the jail, especially of Bengalese, it further appears that the constitution is so feeble, that remedies of an active character are not well borne.

In many of the cases of phthisis, dysentery is represented to have prevailed along with the primary affection of the lungs. Is this really dysentery, or is it not rather the ulcerating and tubercular disease of the ileum and colon, so often observed? It is added, that the dysenteric symptoms often took the place entirely of the originally pulmonary symptoms; and though they seemed

to check, they hardly delayed, and did not avert, the fatal issue of the disease.

It further results, that, at this same Midnapore Jail, the atmospheric temperature varies very much. Thus, the smallest difference between the highest and lowest temperature is stated to be  $7^{\circ}$ , which took place in August 1844. The greatest difference is stated at  $23^{\circ}$ , which took place in December 1844. The highest temperature, we presume, during the day, is  $95$  and  $95.5$ , which is in May 1844 and April 1845. The lowest temperature, we understand, during the day, is  $76^{\circ}$ , which is in December 1844; and the lowest temperature, which we presume must have been during night or towards morning, is  $53^{\circ}$  in the same month of 1844.

It is the opinion of Mr Webb, that pulmonary tubercles and scrofula depend, in India, on bad feeding, bad air, and general bad management. He controverts, however, the doctrine stated by Mr Phillips, that scrofula is a common disease in India. If, he says, we are to understand by scrofula the existence of swellings and scars in the glands of the neck, then this disease is very rare in India. Among the 260 children of La Martiniere, all of Calcutta, not more than thirty being European bred, their ages varying from 4 to 16, and chiefly half-caste, the remainder being European or Armenian; the only children, among the girls, presenting marks of scrofula, belong to one family, and are European bred. Of the boys, two only present marks. In the orphan schools, in which the admissions are between 400 and 500 annually, not one entry of scrofula appears. Mr Webb allows, that, among the natives of India inhabiting the lower range of the Himalaya mountains, he has seen scrofulous swellings and ulcers in the neck rather common; but in no other part of India is he aware that this disease prevails.

Tuberculation in bones is, according to Mr Green, a common form of scrofula with the natives of India. The disease is of an asthenic character, and is represented to be the result less of hereditary taint, than of poor living, exposure, imperfect clothing, and other circumstances connected with poverty and habits thence generated. It seems to affect mostly the bones of cancellated tissue, as the vertebræ, the pelvic bones, and scapulæ, but may also affect the cylindrical bones, as the tibia and fibula, though in these it appears chiefly in the cancellated tissue of the extremities.

Scrofula, when it appears, manifests its effects, in short, more in the periosteum and bones in natives, and even in European bred children, than in enlargement and tubercular transformation of the lymphatic glands. The products of this disease are curdly purulent matter, forming most commonly between the periosteum

and the bone, and occasionally more superficially. In half-caste children, scrofula appears in the form of hip-joint abscess, lumbar abscess, with disease of the vertebræ and similar disorders.

The author concludes with observing, that consumption is so common, and advances with such rapidity, that he regards it as quite a fallacious view to imagine, as many seem to do, that either consumption or scrofula derive relief from the warm temperature of a tropical climate. On the contrary, he states that Bengal, whatever may be said of other equinoctial regions, is most detrimental and fatal to these diseases. All concerned, therefore, he dissuades strongly from sending consumptive invalids to India, or allowing them to remain there. After visiting Penang and Singapore, he is confident that the climate and air of these places in no way retard the progress of consumption, or avert the fatal issue of the disease. In short, he is decidedly opposed to the practice of trusting to the high temperature of a tropical climate for benefit in these.

The Calcutta Museum contains several specimens of *bronchiectasis*, or dilatation and hypertrophy of the bronchial tubes, a disease which appears to take place in India, both in natives and Europeans. Mr Webb is rather disposed to adopt the hypothesis entertained as to the pathological cause and nature of this disease by Dr Corrigan, who, ascribing its formation to the condensation and contraction of the cellular (filamentous or fibrous) tissue of the lung, considers it as *kirrrosis* of that organ. It would not be difficult to show, that this is confounding all distinctions; and however plausible it may at first sight appear, communicates on the nature of the lesion no information whatever. The disease is well represented by Dr Carswell, who regards and describes it as a species or form of hypertrophy.\* The lesion is distinguished by two circumstances. First, the calibre of the tubes is enlarged, sometimes to twice or three times their natural size. They are not, however, uniformly enlarged. At one point they are enlarged, and bulge out into considerable canals. At another they are less so; and at a third they are still less; while at the further extremity they are contracted almost to a point, and sometimes at the tracheal end they are greatly narrowed and flattened. Secondly, the walls of the tubes are thickened and rendered much firmer than in the healthy state. While the first circumstance has caused various observers to look on the lesion as dilatation, the second has led others to look on it as hypertrophy. The important point is, to ascertain what is the cause of this dilatation and hypertrophy; and upon this subject nothing but ideas, which are, to say the least of them, crude and inadequate, have been given. In most cases of well-marked

\* Illustrations of the Elementary Forms of Disease. Fascic. hypertrophy. Plate I. end of text.

*bronchiectasis*, it will be found that the immediate exciting cause is pressure upon one series of bronchial tubes, at their origins or not far thence, and in the course of the tubes. A cluster of enlarged bronchial glands, a small tumour outside the tubes, an aneurismal tumour, or a portion of lung condensed all round them, while the inferior portion is normal, are the most usual causes of dilatation of the bronchial tubes. In certain cases, aneurism of the arch of the aorta or of the *innominata*, when the tumour presses and encroaches much on the lungs at their roots and on the *bronchi*, is a frequent cause of dilatation of the bronchial tubes; and few aneurisms of these vessels take place without inducing this lesion.

In two instances noticed in the present volume the lesion appears to have been the effect of condensation of the surrounding lung, and what the author terms bronchial tuberculosis. A third and fourth instance, the best of all, are given at pp. 187 and 189.

An instance of emphysema deserves notice from its medico-legal bearings, and to show how much need there is of caution on the part of medical witnesses in assigning causes of death in India as well as in Europe.

*Account.*—A man working in a dry dock fell down dead; he was a native carpenter; an European sailor is accused of assaulting and killing him by throwing a stone.

*Autopsy.*—Externally, great fulness of the vessels of the neck, with distended eyes and temples, skin much darker coloured than natural from venous congestion; supposed to have died of rupture of spleen.

*Abdomen.*—Carefully searched, showed nothing unusual but old adhesions in neighbourhood of spleen, and of intestines to abdominal parietes.

*Chest.*—Larynx, slightly cedematous, glottis somewhat reddish, trachea redder and redder as it descended from larynx, till at last it became of an uniform red, or brick colour, and filled, as well as minute ramifications of bronchi, with sputa, watery or serous, somewhat frothy, with, here and there, streaks of yellowish mucus.

*Lungs* blackish, in parts cedematous, the edges especially where resting on the diaphragm emphysematous—cells broken into one another; externally blistered up, with air underneath.

*Head.*—Showed general and excessive venous congestion.

*OPINION AT INQUEST.*—Died asphyxied—whether suffocated from asthma, carbonic-acid gas, or the gunja in smoking, uncertain; but not a mark externally nor a sign internally to show injury from external violence.

*Deduction*, that the five or six respectable Hindoo men who came to swear away the life of an European, were just so many perjured rascals.”—Pp. 153, 154.

This anecdote seems to convey the idea, that Hindoo faith is very much like Punic, modern Italian in courts of law in Italy, and that sort of faith in Westminster Hall where, according to

Jeremy Bentham, the man with the straw in the shoe was invariably seen, ready to assist any one who wanted a willing witness. The truth is, that, whether from ignorance and presumption, or malice, people like the six respectable Hindoos are by no means wanting in countries and towns not a hundred miles from where we at present write.

Many interesting examples of disease of the lungs and appendages are given. But our limits do not permit us to introduce even abstracts of them here; and we would simply strongly recommend them to the perusal of those who are anxious to see what extensive and serious forms of disease may affect the lungs in warm climates, as well as in these cold and occasionally inclement regions. These records ought to convince any one, that changing climate for the cure of chronic pulmonary diseases, or recovery from them, is a most futile and ineffectual remedy. If chronic bronchitis and phthisis cannot be cured, unless by going to a warm climate, and if patients in these diseases cannot recover in the country in which they have been taken ill, it is clear that they have no chance in countries where the temperature is high. We have allowed, that India is, for an equinoctial country, placed in peculiar circumstances; that, with intense solar heat, its atmosphere is liable to be chilled to a very low degree; that in its hilly and elevated regions, with a high temperature where it is warmed by the sun, it is exposed to cold gusts of wind, and great and sudden transitions; and that all these circumstances tend to create pulmonary disease, and aggravate it when induced. Yet what is true of one equinoctial country, is within certain limits true of all; and the conclusion seems to be, that the human race are as liable to pulmonary diseases in and near the torrid zone as in colder latitudes and more temperate regions.

In the general observations on these cases the reader finds some instructive views on the causes of disease and mortality in India. The following remarks on the climate of Simlah deserve attention, as showing the circumstances in which diseases are liable to be produced.

"In a climate like Simlah, which is so nearly allied to that of England, where we are surrounded by European forms of vegetation, we are not surprised to meet with European diseases. In the region of the oak and ivy, the pines, firs, and yew trees; where roses, and jasmines, and the graceful wreaths of the clematis, perfume the air; where buttercups, violets, cowslips, and geranium, are scattered around in profusion; there also is met that deadly scourge of our father-land,—the typhus fever: there also the hill-cholic. But should we proceed to the higher hill ranges, we have a more ardent sort of fever, and if we go to the lower ranges, one of a more deadly type is found. Whilst the most fatal type of jungle fever destroys the very letter carriers in their passage through the



*Terai*, at the foot of the hills, and cholera prevails in the plains ; the Mah-murree, or plague, ravages some of the northern parts of the Gurhwal district. In a letter which I received at Simlah, it is thus mentioned by Captain Huddleston, who has charge of the district of Gurhwal : 'The mortality from this malignant fever is very great indeed, and whole villages are half depopulated. Only a few days ago I got a report of its having broken out in one village and eleven people dying from it,—all the others fleeing to the woods and caves. The *symptoms are all those of the plague*: save that they are confined to two pergunnahs, chiefly Budha, along the banks of the Piridah river, and up the sides of the mountains, and Nag-pore. The Europeans, or pilgrims, are never affected ; yet the disease has now been raging for years up in these parts. It has been known to extend its usual limits, though very seldom. The people die in two or three days, and have *large swellings over their bodies* ; rats, snakes, &c., die, they say first, before its breaking out in any particular village ; then the men are affected. I have at this moment several villages quite abandoned on this account, and the people have not yet returned to their homes, but are still living in the woods and in caves, and villages close by, that they may attend somewhat to their cultivation.'

"A wide field opens to us for reflection in this short account of the Mahmurree. It might suggest some very practical ideas to those who enforce quarantine. A more full investigation would probably show this disease to be the same with the malignant typhus fever, which has been desolating the villages in the neighbourhood of Simlah ; for in the worst cases brought to the hospital, buboes were met with, both in the groin and axilla.

"To show the probability, however, of this disease being only a modified typhus, it will be necessary to consider the influence of climate. Climate on these hills greatly depends upon elevation. In fact, this elevation comprehends *air*, temperature, vegetation, season, and water, all of which powerfully modify disease. Perhaps there is no country in the known world where a man may so rapidly pass a variety of climates, as in the Hymalayan mountains. These hills differ from those of Europe very remarkably, in having no lakes to fill up the valleys. Some of them, from their towering summits capped with snow, and bearded with bare, gaunt-looking pines, go down to the deep valleys, where you meet the willow leaning over some brawling stream ; and lower still, the lime-trees, plantains, cactus, and wild date. Midway may be the walnut, mulberry, and perhaps the pomegranate ; a little higher still, rhododendrons, oaks, and ivys unglazed, with wild cherries, raspberries, currants, and firs. Then comes the prickly oak, very like our holly, and well coated with varnish, and further defended also by lichen, moss, and long hanging dharrees ; then pines and glazed firs conduct us to the top. It is obvious therefore, that, in the course of this mountain slope, many zones of temperature, with corresponding zones of vegetation, will be passed in a few hours. I have been in

the valley of the Jumna, with the thermometer 110°; the same day I have arrived at masses of unmelted snow.

"That disease should be greatly influenced by such broad differences, will not surprise us. I found that a line drawn at a certain height at Simlah, on a level with the bazaars, would have all the petechial, or low typhoid cases below it; that a mixed fever would correspond with this line, whilst that the bare and lofty summits of the hills are entirely free from fever. Every fatal case of fever was below this imaginary line, and also every case of cholera; whilst Captain Huddleston's letter would lead us to believe that the 'Mah-murree' has chosen its seat far below, in the region of the cactus and wild date, and that it has never looked at the oak, the ivy or rhododendron.

"A further confirmation of these views, as to the effect of air, of decaying vegetation, and elevation upon disease in the hills, was furnished by Daboo, a very intelligent lad, one of the apprentices of the Simlah hospital, who executed the arduous duties upon which he was despatched with a zeal and fidelity which I never saw equalled in a native. The ravages of the fever were so fearful about two or three marches from Simlah, that the Assistant Political Agent requested medical aid should be afforded; and this lad was sent out, with instructions and medicines, and, aided by the countenance of the civil power, was very serviceable in arresting the disease. He had about 300 cases under his charge. He assured me that the disease would often *pass by* a village situated on a bare hill; that all the spotted cases came from low situations, and were often infectious. A man fleeing from a village where the disease was raging would sicken in a village to which he had fled, and that there the fever would often prevail generally. But he attributed the infectious nature of the disease to *their crowding the sick together in the filthy rooms* occupied by their cattle, in the lower part of their houses, and leaving them unheeded to their fate:—husbands deserting their wives, and children their mothers. He, on the contrary, *insisted upon fresh air*, and removal to the upper rooms, quieted the excessive fears of the people, and, by medicine and simple treatment, recovered most of them. He said the more bare parts of the hills had agues and common fever, quite different from the spotted fever, which he considered to be decidedly infectious."—Pp. 212\*—214.\*

We doubt whether the distinction now mentioned would stand the test of strict examination. The presence of spots is no proof that the disease, in which they appear, is typhus; for spots are not uncommon in remittent fever, or continuous remittent (*amphimerina and tritaephya*), which seems to be the native product of this part of India. We must, however, allow Mr Webb to deliver his views in his own way. Yet the following statement throws on the contagious character of the disease considerable uncertainty.

"Thus, I found at the Simlah hospital, such cases of spotted petechial typhus fever as were brought up from below would run through their course, but never communicated disease to others in the same ward. That common continued fever, which prevailed about Simlah, did not merge into typhus at the hospital, but in the houses of the European residents, surrounded with humid vegetation, it did. One lady died in consequence, another very narrowly escaped death. I found bad, neglected cases, fatal under any treatment, often dying with buboes in groins and axilla."—Pp. 214.\*

Much multifarious matter follows; the black hole of Calcutta and its victims; foul air in general; sulphuretted hydrogen and its effects; cholera, typhus, gangrene, and plague; gangrene of the brain in typhus; the Old Bailey cases; then the air of Calcutta and its contamination by open drains and the want of sufficient, covered drains; and the effects of bad air on children.

It appears that in Calcutta, the sources of contamination of the air are as numerous and concentrated as in London. The houses, or kennels rather, of most of the lower classes of inhabitants are built on the ground, without excavation or any means of keeping out the telluric exhalations. The floors are earthen; and near these hovels are gutters or open drains for receiving all the filth and used water of these places. Near these receptacles of every thing unclean the inhabitants place their beds, if beds they can be called, which are mere mats or settles covered by a mat or rug; and in these they are crowded together in great numbers, breathing air, where the temperature during the day is, from 80 to 90°, and which air has been breathed over and over again by persons in similar circumstances. Drains, that is to say proper underground covered drains, can hardly be said to exist. The moisture of the open gutters is evaporated by the heat; and any that is left is sufficient to promote the progress of decomposition in the other matters, animal and vegetable, which are left in these open drains; and the result is, that the inhabitants are exposed to the incessant operation of an atmosphere loaded with all sorts of impurities.

It would have added much to the value of the information communicated in this volume, had it been practicable to give the population of Calcutta, pure Hindoo, mixed blood, Moslem, and English, with such approximative estimates of the Georgian and Armenian population as it might be possible to obtain; and also bills of mortality, stating the deaths and the reputed causes. On these points no information is given, and hence we are left very much in the dark as to the diseases most prevalent and most fatal.

The only information which makes any approach to this is, that it is stated, first, that the number of subjects brought into the dissecting rooms of the medical college of Calcutta, for the ten

years, from 1837 to 1847, nearly amounts to 3500, or at the rate of 350 annually; and that among 460 subjects brought to the college, the diseases which were stated to cause death, were registered in the following manner: remittent fever, 40 persons; acute and chronic dysentery, 60 persons; intermittent fever, accompanied with organic diseases, 175; cholera, 100 persons; diarrhoea, 20 persons; rheumatism, 20 persons; phthisis and chronic bronchitis, 13 persons; and sexual diseases, 32 persons. From this, it appears that among the lower and the most indigent ranks, the prevalence and fatality of disease stands in the following order; intermittent fever, cholera, dysentery, remittent fever, diarrhoea and rheumatism, and phthisis and chronic bronchitis. As to the diseases called sexual, we have no means of knowing whether all were in females, or whether they took place in both sexes. Ten are stated to be secondary venereal affections. The residual twenty-two were evidently in females, as seven are set down to abortion, ten to puerperal fever, and five to gangrene of the womb. It is only to be observed, that the whole of these orders of disease are the offspring of air contaminated by various impurities, and liable to sudden alternations of temperature.

The further examination of this work, however, our limits permit us not at present to continue. We have given a view of the subjects treated in the first part, and whatever is to be said on the second part, must be deferred to a subsequent opportunity. The volume contains, it must be seen, much important and valuable information; and though, on various points, deficiencies may be observed, yet, taken altogether, it is highly creditable to the diligence and zeal, as well as the knowledge of the author. For what he has done, Mr Webb is certainly entitled to thanks and commendation, and his work will rectify many erroneous impressions current in Europe regarding the diseases prevalent in India.

It is proper not to forget to say, that this work is in reality much more extensive than it appears to be. It contains a very great number of duplicate pages, which must increase the work to double its nominal amount.

**ART. II.—1. *Recherches sur la Digestion.* Par MM. BOUCHARDAT et SANDRAS. (Annales de Chimie et de Physique. Troisième Serie. T. vième. Paris, 1842. Pp. 478.)**

***Researches on Digestion.* By MM. BOUCHARDAT and SANDRAS. (Annales, &c.)**

**2. *On the Nature and Treatment of Stomach and Renal Diseases; being an Inquiry into the Connection of Diabetes, Calculus, and other Affections of the Kidney and Bladder, with Indigestion.* By WILLIAM PROUT, M. D., F. F. S., Fellow**

of the Royal College of Physicians. The Fifth Edition, revised. London, 1848. 8vo, pp. 595. *Book III., Comprising an Outline of the General Physiology and Pathology of Assimilation; and of the Secretion of Bile and Urine.*

3. *Du suc Pancreatique, et de son role dans les Phenomenes de la Digestion.* Par le Dr CL. BERNARD, Professeur d'Anatomie et de Physiologie Experimentale, &c. Memoire lu a la Societé de Biologie. Extrait des Archives Generales de Medecine. Paris, 1849.

*On the Pancreatic Fluid, and the part which it performs in the Phenomena of Digestion.* By Dr CL. BERNARD, Professor of Anatomy and Experimental Physiology. (Archives Generales de Med. 1849.)

4. *Rapport sur un Memoire de M. Cl. Bernard, intitulé Recherches sur les Usages du suc Pancreatique dans la Digestion.* Commissionnaires, MM. Magendie, Milne-Edwards, Dumas, Rapporteur.

*Report on a Memoir by M. Cl. Bernard entitled Researches on the uses of the Pancreatic Fluid in Digestion.* Committee, MM. Magendie, Milne-Edwards, Dumas, Reporter.

FOR seven or eight years past, various inquirers have endeavoured to render more precise and accurate than previously, our physiological knowledge on the digestive transformation of certain articles of food, and to determine the peculiar nature of several of the separate stages in the process of digestion. Among these the changes effected in alimentary articles, first, by mastication and admixture with saliva, and, in a subsequent stage, by admixture with the pancreatic fluid, have occupied much research, both chemical and physiological. To the facts ascertained as to the peculiar effect of admixture with saliva we have already more than once directed attention; and, as occasion offered, to those effected and represented to take place under the action of the pancreatic fluid.

In 1842, MM. Bouchardat and Sandras published an interesting memoir on digestion, in which they examined carefully the changes undergone by various sorts of alimentary articles during the successive stages of the digestive process.\* Of these experiments, a summary was given in the fifty-ninth volume (p. 216) soon after their publication.† In volume sixty-fourth were noticed the experiments of M. Mialhe, tending to show, that the digestion

\* *Recherches sur la Digestion; par MM. Bouchardat et Sandras. Annales de Chimie et de Physique, 1842. 3ieme serie. T. v. 478.*

† *Edin. Med. and Surg. Journal, Vol. lix. 1843.*

of amylaceous and saccharine matters was effected chiefly by the saliva, and especially by means of a principle which he had discovered in that secretion; which he found to be in composition and properties similar to *diastase*; and which he denominates animal diastase or salivin.\* To this principle belongs the property of dissolving starch, and finally converting the solution into dextrine and glucose.

In volume sixty-fifth were published the experiments of Dr Robert Dundas Thomson, on the digestion of vegetable albumen, fat, and starch, which show that dextrin and soluble starch are present in the stomachs of animals fed on farinaceous substances, during and some time after digestion, and that sugar exists in the blood of animals which have been fed on starch.† In volume seventieth were noticed several experiments by M. Cl. Bernard, tending to show, that salivary fluid may exert the power of converting starchy or amylaceous articles into saccharine matter or glucose; and, in adverting to certain experiments by Magendie on the chemical properties of different forms of saliva in the horse, we took occasion to make some general remarks on the researches of all these inquirers, as tending to throw light on the digestion of amylaceous articles of food.‡

M. Cl. Bernard has now published, on the pancreatic fluid and its effects on the process of digestion, a series of experiments which deserve the attention of the physiologist. It may be proper, in order to give a connected view of the whole, before detailing them, to revert to the experiments of MM. Bouchardat and Sandras, with which they are in some degree related.

1. These chemists ascertained, as had previously been done by Dr Prout in 1824, that fibrine and fibrinous articles of food are dissolved in the stomach by means of a fluid there secreted; that the agent of this solution of fibrine is hydrochloric acid in a diluted form; that the dissolved fibrine in the stomach presents a well-marked acid reaction; that in the duodenum this acidity disappears, and there is rather a slight alkaline tendency; that the secretion of the digestive liquor in the stomach is either more abundant, or its acidity is more marked after food has been given than in the fasting state; and that the fibrin thus dissolved is absorbed into the circulating system, not, as originally taught, by the chyloferous vessels, but directly by the venous capillaries.

Dr Prout has modified his earliest opinions by ascribing the solution to chlorine.

2. These chemists ascertained that gluten, which forms an important part of alimentary articles, is digested much in the same

\* Edin. Med. and Surg. Journal, Vol. lxiv. p. 266. July 1845.

† Ibid. Vol. lxx. p. 505. April 1846.

‡ Ibid. Vol. lxx. pp. 245, 246, 480. Edin. 1848.

manner as fibrine. As, in the case of fibrine, the solvent agent of gluten is hydrochloric acid in a very diluted form; the matter thus dissolved is immediately absorbed by the orifices of the veins; and the digestion of gluten is commenced and completed within the stomach.

White of egg boiled, and boiled meat, are not dissolved in greatly-diluted hydrochloric acid. Though the presence of hydrochloric acid be necessary to the solution of these substances, there is further required the simultaneous presence of a particular matter produced in the stomach of living animals.

This substance is most probably pepsin, or some analogous animal matter.

This differential character presented by fibrin and albuminous matters of undergoing easy solution in greatly-diluted hydrochloric acid, before being boiled, and of not being dissolved in this acid, after being subjected to the action of boiling water, these chemists regard as most important. The fact proves that the molecular change taking place in albumen by boiling, takes place in like manner in fibrin, and may be regarded as a general character of albuminous matter. This distinction is important in establishing a discovery peculiar to the authors, namely, that of the solution of albuminous matters by dilute hydrochloric acid.

The energy of the solvent properties of the gastric fluid has been known since the experiments of Reaumur, Spallanzani, and Stevens. Prout, Tiedemann, and Gmelin proved in this fluid the existence of hydrochloric acid. These last observers further found that acetic or lactic acid, acetate of soda, lactate of soda, and dilute hydrochloric acid, possess solvent properties. MM. Bouchardat and Sandras nevertheless inferred that the knowledge of the inquirers now mentioned was not precise as to the solvent action of this acid, because they represent hard white of egg to be dissolved by it, like fibrin, while this solution is effected only when a concentrated acid is employed. This well known property of hydrochloric acid differs completely from that specified in the memoir of MM. Bouchardat and Sandras.

3. *Starch and Amylaceous Articles.*—In that class which M. Dumas proposed to call aliments of respiration, starch occupies the most important place. The large proportion in which starchy principles enter into the food of men and animals, shows that it is a most necessary article of nutriment.

All the experiments made by these chemists prove that starch is not habitually converted into sugar under the influence of digestion; and the saccharine matter observed in the experiments of Tiedemann and Gmelin, they think, may have been produced by the action of the leaven or ferment on the extractive matters which they examined.

It appears further, that, during the digestion of starch or *fecula*, no dextrine is produced.

These chemists always ascertained a proportion greatly more considerable of lactic acid after a meal of amylaceous matters, than when the animal has been killed fasting, or after a meal of fibrin or of gluten. They therefore consider it to be demonstrated, that, out of the period of digestion, starch is converted into lactic acid or a lactate, both soluble in water, and which may therefore be absorbed immediately, as drinks by the orifices of the veins, and cannot come into the system by the channel of the chyliferous vessels and thoracic duct, as has been hitherto believed.

In truth, the comparative analysis of the chyle of an animal fasting, and of an animal killed after a meal of amylaceous matters, presents no difference. In this chyle no soluble starch is contained. They observed that the chyle was almost neutral in dogs, into the stomachs of which *fecula* had been injected; and that, on the other hand, it was sensibly alkaline in animals fed with bread. They think it established, that the products of starch contribute in no respect to the formation of chyle, unless the physiologist chooses to admit the existence of these mysterious transformations of starch into albuminous matters,—an inference which is authorised by no fact at present known.

4. *Fat*.—Fatty substances perform an important part in the nutrition of man and of carnivorous animals; and it is interesting to understand the mode in which they are assimilated.

The experiments performed by MM. Bouchardat and Sandras show that the digestion and assimilation of fat are not accomplished in the stomach; that article undergoes within this organ no transformation, no elaboration. It is in the duodenum that this principle undergoes the modifications which are to facilitate its absorption. These modifications are simple. The fatty matters are mixed with the bile and the pancreatic fluid, are divided and converted into an emulsion without changing their chemical characters. If they contain free margaric and oleic acids, these acids are neutralised by the alkali contained in the pancreatic fluid and in the bile.

When once converted into emulsions by the bile and pancreatic fluid, fatty substances are immediately absorbed by the orifices of the chyliferous vessels, and thence conveyed into the thoracic duct and mixed with chyle. On this point the analysis of the chyle of animals fed on fat substances leaves no doubt. This absorption is continued through the whole alimentary canal, and when the proportion of fatty matters is greater than is requisite, it is expelled with the excrements.

These authors recapitulate the following as general deductions from their experiments.



1. In digestion, the function of the stomach consists as to albuminous matters, namely, fibrine, albumen, caseum, and gluten, in dissolving them by means of hydrochloric acid.

2. This acid is sufficient, though diluted to a half thousandth part,\* for the solution of the before specified matters, while raw. If they have undergone boiling, dilute hydrochloric acid no longer dissolves them within glass vessels; and as they are found dissolved in the living stomach, they infer that there takes place within the living stomach, some other process than simple solution by dilute hydrochloric acid; only the presence of hydrochloric acid appears to be in all cases indispensable.

3. As to albuminous matters, digestion and absorption take place almost exclusively in the stomach; the rest of the alimentary canal no longer performing this solution, the abundance of which within the stomach has been already established.

4. In the stomach also takes place the solution of amylaceous or starchy articles. This principle appears, according to them, not, in the ordinary state, to be converted into sugar. They think it not sufficiently proved that it passes into the state of soluble starch. Its conversion into lactic acid they regard as established.

5. The absorption of this form of alimentary articles appears to them not less exclusively confined to the stomach than that of the solutions of albuminous matters; which would accord with the peculiar dispositions of the intestinal tube in animals not carnivorous.

Fat undergoes no change in the stomach. Within the duodenum it passes into the state of emulsion by means of the alkalies furnished by the liver and pancreas. This emulsion is found in abundant quantity in all the rest of the intestine.

All these facts, simple and precise, they regard as established with absolute certainty. Another equally conclusive is the following.

7. The chyle appears to them to be less abundant, yet similar, in animals killed fasting and in those which have been fed on albuminous and amylaceous matters; it presents a marked difference only in those fed on fat. This proximate principle is found in chyle in considerable quantity.

From these facts, they deduce a theory of digestion which they regard as simple and natural.

They consider the substance or product called chyme by physiologists as imaginary. The substance so designated they consi-

\* It is not easy to say whether by this term the authors understand a five-hundredth or a two thousandth part. The half of a thousandth part is, correctly speaking, a two thousandth part. But if by half a thousand part be meant the part of half a thousand, then it must be the five-hundredth part, at which the degree of dilution is fixed.

der as a mixture, consisting of the remains of alimentary articles, not dissolved, but the solution of which is to be continued slowly within the intestinal convolutions, of excretions from the glands and the intestinal mucous membrane, destined to form subsequently excrementitious matters,—and a pulpy mass particularly prepared for assimilation.

As to chyle, it has been hitherto supposed that alimentary articles dissolved at first within the stomach, afterwards precipitated and converted into chyme, passed into the chyle in a state of minute division or new solution. But this inference is at variance with the fact that fibrine coloured does not furnish coloured chyle.

Chyle collected during the digestion of starch has the same composition nearly as that collected during the digestion of fibrin.

The experimenters propose therefore the question, whether albuminous aliments, namely, fibrin, caseum, gluten, albumen, like amylaceous aliments, are not converted into chyle, as has been hitherto represented; and further, what is the part of the chyloferous apparatus and of the chyle, the largest production of which is unquestionably during digestion?

Experiment seems here to answer, that the orifices of the chyloferous vessels are intended to absorb fatty aliments converted into emulsion by means of bile. But it is probable that to this bowel the part of a production so important as that of chyle is not confined; and the interpretation which they put on the facts observed is the theory of digestion which they offer.

This theory is substantially the same with that previously proposed by Dr Prout, and which consists in this; that when sapid articles of food are presented to a hungry animal, saliva flows abundantly into the mouth, and gastric fluid into the stomach; that the latter contains acids which are furnished from the blood by decomposition of its salts, namely, chloride of sodium and lactate of soda; that this decomposition necessarily leaves in the blood free alkali, which must either remain in that fluid or be separated from it; that this alkaline matter is separated by the abdominal glands, and thus neutralizes any acidity in the alimentary matter before it is received into the blood.

This theory was proposed in 1840 in the third edition of his work on Stomach and Urinary Diseases, by Dr Prout;\* who even went so far as to point out its probable connection with galvanic agency; and consequently it is impossible to regard it as new when proposed by MM. Bouchardat and Sandras. It certainly looks as if the secreting system of the gastric tubules, on the one hand, formed the positive end of a galvanic apparatus, while the liver and pancreas, on the other, formed the negative pole.

One inference, drawn by MM. Bouchardat and Sandras, is

\* Introduction, p. xxiii. parag. 47 and 48, and xxiv. parag. 49, 50, and 51, .

liable to doubt and uncertainty. Though it may be the case, as they infer, in certain circumstances, that, when the proportion of fatty matters is too considerable, the portion not digested is eliminated with the excrements; yet this does not appear in all instances to be the fact. The experiments of M. Gluge and Thierresse, given in our sixty-third volume, p. 511 (1845), show that fatty matters, which are not digested, are not always eliminated. They are taken, according to these experiments, into the vascular system as fatty matters; they circulate with the blood, and they in no long time induce a peculiar species of pneumonia, which, with great certainty, destroys the functions of the lungs, and causes the death of the animal. There is little doubt that the presence of fatty matters in the human body is as detrimental as in the bodies of animals.

It was, nevertheless, an important step in elucidating the phenomena of digestion, to show, that fatty substances undergo no change in the stomach, and only begin to be digested in the duodenum, where they meet with the alkaline secretions from the liver and pancreas.

M. Cl. Bernard has added to the precision and accuracy of this theory, by several experiments on the uses of the pancreatic fluid, and its part in the function of digestion. He undertakes to prove that the pancreatic fluid alone, to the exclusion of all the other intestinal liquids, has assigned to it the faculty of specially modifying,—in short, of digesting,—the neutral fat substances contained in the food, and in thus rendering them fit to be absorbed by the chyloferous vessels.

M. Bernard has in this inquiry kept two leading objects in view; first, to determine the physical and chemical properties of normal or healthy pancreatic fluid; and, secondly, to ascertain what changes it effects on alimentary articles, and on what alimentary articles it acts. In prosecuting this inquiry, he has performed seven experiments on dogs and rabbits; but these experiments he has varied in every possible mode during the space of two years. The number of dogs on which experiments have been during this time performed, amounts to thirty-four. The details of these experiments, it is unnecessary to repeat. A general view of the results obtained, supposing all the experiments correctly performed, and their results faithfully recorded, is all that is in these pages necessary.

Pancreatic fluid may be distinguished into two sorts; normal or healthy pancreatic juice, and morbid pancreatic fluid. Normal pancreatic juice is that which is obtained in good condition from an animal, before the pancreas has been attacked by inflammation, or collected from a dog with an old-established pancreatic fistula. Morbid pancreatic fluid is that which is secreted in great abun-

dance at the time when the symptoms of inflammatory reaction appear in the pancreas, and in the wound in the abdomen, inflicted in order to procure it.

Normal or healthy pancreatic juice is a colourless, limpid, viscid, or ropy liquor, flowing slowly in large pearly or syrupy drops, and becoming frothy upon agitation. This liquor has no characteristic odour. Placed on the tongue, it communicates the sensation of a viscid liquor; its taste is a little saltish, like the serum of the blood. M. Bernard uniformly found the pancreatic juice to give a manifest alkaline reaction; never in any instance was it acid. Normal pancreatic liquor, exposed to heat, is coagulated and converted into a white concrete mass. The coagulation is entire, and complete as if it were white of egg. All becomes solid, and not one drop of liquid remains. This white matter of the pancreatic juice is in like manner precipitated by nitric acid, by sulphuric acid, and by concentrated hydrochloric acid. The metallic salts, pyroligneous acid, and alcohol, further cause the complete precipitation of the organic matter of the pancreatic juice. The acetic, lactic, and dilute hydrochloric acids, do not coagulate pancreatic juice. Alkalis produce in it no precipitate, and they re-dissolve its organic matter when that has been previously coagulated by heat, the acids, or alcohol.

These facts show that pancreatic juice resembles, as was taught by M. Magendie, MM. Tiedemann and Gmelin, and others, the albuminous liquors of the animal body. There is, nevertheless, no analogy physiologically between pancreatic juice and albuminous liquid. M. Bernard afterwards proves that this coagulable matter is the active principle; and he therefore is obliged to infer, that the matter of pancreatic juice is not albumen physiologically, but must be something different. Pancreatic juice and albumen are indeed chemically different; and one of the characters of this difference is, that when the matter of pancreatic juice has been coagulated by alcohol and then dried, it is entirely and easily redissolved in water, whereas albumen treated in the same manner is not again perceptibly redissolved in water.

Morbid pancreatic fluid is a liquid of watery consistence, void of viscosity, colourless, but often opalescent, and sometimes of a reddish tint. This liquid has a taste at once salt and nauseous; its reaction has always appeared alkaline; its density lower than healthy pancreatic juice. Treated by heat and acids, it is not coagulated.

The conversion of normal pancreatic juice into morbid pancreatic fluid does not take place suddenly; it is accomplished gradually; so that between the characters assigned to normal pancreatic juice and those belonging to that which is morbid, many intermediate shades may be found. These variations, never-

theless, do not act upon the presence of the active coagulable matter, which is very abundant in the pancreatic juice, first drawn after the operation properly performed, while the proportion of this same matter diminishes progressively in proportion as the interval after the operation is increased, and it may be completely wanting when inflammation has taken full possession of the pancreatic tissue. As this matter disappears, the pancreatic juice becomes more watery, and loses its activity. It may on the whole be said, that the pancreatic juice is so much more healthy and more active, as it is more coagulable by heat, and that it is so much more inert and more altered as it is less coagulable.

Pancreatic juice is, of all the liquids in the economy, the most changeable. When normal pancreatic juice is exposed to a low temperature,  $23^{\circ}$ — $14^{\circ}$  Fahr., it may be preserved for several days; and then it is observed, that by the depression of temperature the viscosity of the liquid is increased, and that it becomes of a consistence analogous to that of soft jelly. If, on the contrary, pancreatic juice is kept at the temperature of  $104^{\circ}$ — $113^{\circ}$  Fahr., it is speedily modified, and at the end of some hours it is completely altered, that is, it exhales a nauseous odour, it furnishes a cloudy deposit, and loses the property of coagulating by heat. The alkaline reaction of the liquid continues in these circumstances. During the heat of summer, and in tempestuous weather, this change in the pancreatic liquid is sometimes effected in a few seconds. It is therefore requisite to keep cool the pancreatic juice and the animal from which it is obtained, since the fluid may be changed in the caoutchouc bladder made to receive it, while attached to the extremity of the silver tube. The deposit which is produced at the time when the fluid is altered, has sometimes presented a peculiar silky appearance. M. Bernard always found in these circumstances, by the aid of the microscope, a great amount of needle-like crystals of margarine or of margaric acid.

The leading characters now given of pancreatic fluid belong to that secretion not only as obtained from dogs, but as found in rabbits, horses, fowls, and pigeons.

The knowledge of the variations above stated enables us to understand the cause of the discordant statements of authors on the amount of albumen contained in the pancreatic juice. The distinction made by M. Bernard of pancreatic juice into normal or healthy and altered or morbid, is not only useful in studying the physical and chemical properties of this fluid; it is indispensable in explaining its physiological or digestive properties, which is at present the most important point.

To determine the nature of these properties, M. Bernard di-

gested for some hours neutral fat substances with pancreatic fluid, and obtained the following results.

When neutral fat matters, of animal or vegetable origin, are left in closed vessels with normal pancreatic juice at a temperature of from 95° to 100° Fahrenheit, they are converted into a whitish cream-like emulsion; and, though no separation is at first manifest, it is found, after some hours, that, under the influence of the pancreatic juice, the fat has undergone a chemical change. At first, at the moment of mixture, the neutral fat matter and the alkaline pancreatic juice form an emulsion with alkaline characters; five or six hours afterwards, the mixture presents a decided acid reaction. The fat matter is then converted into glycerine and fat acid.

To pancreatic juice alone belongs this property. M. Bernard subjected various fat substances employed as articles of food to the action of bile, saliva, gastric juice, serum, and the cephalorachidic fluid in similar circumstances, and without obtaining any similar results. Olive oil so treated never underwent any degree of change.

To accomplish this change, further, M. Bernard shows that pancreatic juice must be in the normal state, and quite healthy. When morbid pancreatic juice is employed, it exerts no action on fat matters, and a separation between the inert pancreatic fluid and the unchanged fat or oil speedily takes place. It appears also that, if the alteration in the pancreatic fluid is incomplete, and that if it still is a little coagulated by heat, its action on fat takes place but in an imperfect manner.

M. Bernard, therefore, argues that, from these experiments it results, that pancreatic juice, by converting into emulsion and chemically modifying fat substances within the intestine, renders them capable of absorption, and in this manner becomes the sole and essential agent in the formation of the white homogeneous liquid which circulates in the lacteals, and which is known by the name of chyle. This term, as well as that of chyme, he regards in the present state of physiology as denominations entirely unmeaning. All that he allows is, that the lacteals contain a white homogeneous milky liquid, only by virtue of their absorbing from the intestine fat matters therein changed, and that the limpid transparent chyle, improperly named by some authors vegetable chyle, is in his judgment chyle without fat, while white milky homogeneous chyle, called by the same authorities animal chyle, in contradistinction to the preceding, is chyle containing fat converted into emulsion, and chemically modified.

M. Bernard has, in prosecuting these experiments, found that when dogs were put to death during the process of digestion, fat articles of food were merely rendered liquid in the stomach by

the influence of temperature. In the duodenum, on the other hand, below the opening of the pancreatic duct, fat could no longer be distinguished by its proper characters. The alimentary mass was a homogeneous pulpy, creamy emulsion, coloured yellow by bile. The lacteals were further distended by this creamy matter. When, on the other hand, the pancreatic ducts were tied, the smallest of which opens near the *ductus choledochus*, while the large one opens in the duodenum two centimetres lower down, he ascertained that fat remains unchanged in the small intestine, and that the chyloferous vessels contain nothing but a limpid fluid void of fat, which has not been absorbed, in consequence of the pancreatic juice having been withdrawn.

Though this experiment may seem conclusive, the author appeals to another in the rabbit. In that animal the pancreatic duct which is single, opens in such a manner in the intestine, that the arrangement seems to anticipate the desires of the physiological inquirer. The duct opens low down in the intestine at the distance of 35 centimetres, equivalent to 13 inches English, from the *ductus choledochus*. When rabbits are made to eat meat or fatty substances, the fat passes unchanged into the intestine, and undergoes no modification until it reaches the outlet of the pancreatic duct. At this point, the chyloferous vessels, which above contained transparent chyle, appear filled with white milky fluid. This simple experiment the author regards as quite decisive.

An objection liable to be raised against this hypothesis, M. Bernard next answers.

It is well known that Sir Benjamin Brodie is believed to have shown, many years ago, by experiment, that the bile has the property of digesting fatty matters. M. Bernard answers, first, that the main source of the error is the opinion entertained by physiologists, that the pancreatic juice is identical with the saliva; and, secondly, that the experiments of Sir B. Brodie were contradicted by the results obtained by Magendie in repeating them.

Sir Benjamin Brodie observed, that, on tying the *ductus choledochus* in cats, the lacteals no longer contained fat substances, and that the chyle was limpid and transparent. M. Magendie repeated this experiment, and found on the contrary, that, notwithstanding the absence of bile in the duodenum and jejunum, fat was converted into an emulsion, and the chyloferous vessels contained white homogenous milky chyle.\* Of these experiments and their discordance, M. Bernard gives the following explanation.

In the cat, the principal pancreatic duct anastomoses with the *ductus choledochus*, before opening into the intestine, so that he infers, that Sir B. Brodie, keeping in view only the action of the bile, and attaching no importance to the pancreatic duct, may have tied

\* Journal de Physiologie, Tome iii.

it along with the *ductus choledochus*; and in this manner is explained very well how fat was not converted into emulsion, and why the chyle was limpid and contained no fat matter. M. Magendie performed his experiments on dogs, animals in which the *ductus choledochus* is completely separate from the two pancreatic ducts. From this it results, that the excretion of the pancreatic fluid being unimpeded, fat would continue to be converted into emulsion, and chyle, white, milky, and homogenous, to be formed. These experiments are therefore correct on both sides. The difference in the results is explained by the peculiar disposition of the insertion of the pancreatic ducts in the kinds of animals employed for the experiments. These facts, therefore, cannot be said to contradict each other, on the theory which M. Bernard supports; and both tend to give it confirmation.

The pancreatic juice produces its peculiar effect on neutral fat substances by virtue of a peculiar organic matter which it contains. It is not simply because it is an alkaline secretion that it contributes to the digestion of fat dietetic articles. If so, its place, when wanting, might be supplied by diluted alkalis. Besides this alkaline property, there is something which, when the alkaline principle decomposes and modifies the fat articles, enables them to be further transformed and animalised. This other principle is manifestly an animal principle approaching to albumen, yet not identical with that element.

It is also to be observed, that the results obtained by M. Bernard by no means invalidate those already obtained by MM. Bouchardat and Sandras, who maintain that starch is converted into glycose by means of pancreatic fluid. All that M. Bernard observes on this point is, that the action of pancreatic juice on amylaceous articles of food is by no means special; that is, amylaceous articles are transmuted and reduced under the influence of various other animal liquids. In short, this property belongs, as has been already shown by the author and other inquirers, as Magendie, Mialhe, and Rayer, to the mixed saliva of man and animals, to the serum of the blood, and to all the alkaline liquids of the economy. M. Bernard adds that this conversion of starch into glycose takes place as completely under the influence of altered or morbid pancreatic fluid, as under that of healthy and normal pancreatic juice.

The most important inference, in a physiological point of view, resulting from all the experiments and observations made by different inquirers, is this. The secreted liquids employed in the several stages of the digestive process are distinguished by possessing not only chemical, but vital, properties. By virtue of their chemical properties, they effect on the articles employed as



food, certain chemical changes, which are preparatory and necessary to the subsequent vital changes. Thus gastric juice not only, according to the best authorities, contains chlorine, by means of which it effects certain chemical changes in the food; but it further contains some organic principle allied to albumen, yet not albumen, by means of which it assimilates, and more perfectly animalizes the chemically-changed food. This principle has been described as pepsin by Schwann, and by others has been simply considered as a peculiar organic principle. In like manner, bile contains free soda and sulphur, all of which seem to exert peculiar chemical influences, and to produce peculiar chemical changes. But bile also possesses some peculiar animal or organic principle, whether taurine, bilin, or any other name be given it, which lends its aid to the further conversion of the alimentary mass. Pancreatic juice has been shown to be not only alkaline, and to effect, in consequence of this alkalinity, important changes in fatty and oily articles of food, but it possesses also some peculiar animal or organic matter, albuminoid, not albuminous, and which further animalises and assimilates the oleaginous and adipose matters which had previously been changed only chemically. It appears, in short, that, for the purpose of assimilation, or conversion into part of the animal body, an organic principle, with vital properties, appears to be indispensable; and that, though the chemical changes are important, they do not constitute the whole of the process of assimilation.

These results are not less important physiologically than pathologically. It is manifest, that while it is most important to attend to chemical changes, and to see that, so far as we have it in our power, they be rightly performed; yet not less necessary is it, to look to the state of the organic principles, and to see that they are rightly secreted, and are not compelled to do a greater amount of duty than that to which they are adequate.

Lastly, it may be inferred, that, if, as Haller and many other physiologists have taught, the intestinal membrane secretes a fluid necessary in the ulterior stages of assimilation, that fluid must equally possess chemical principles and organic principles, and that, while the former are of use in carrying on chemical changes, or preventing, as it may be, improper chemical changes, so the vital or organic principle is employed in assimilating and animalising alimentary matters which have undergone previous chemical changes.

These views on the uses of pancreatic juice are, in a physiological and pathological view, very important. It has been long observed, that in certain morbid states of the system, fatty matters are discharged by the bowels; and various physicians, especially Dr Bright and Dr Elliotson, inferred some connection between

this phenomenon and the state of the pancreas.\* Conversely, the pancreas has been by various observers, in certain forms of bad health, found to be more or less diseased. Supposing the doctrines now delivered regarding the use of the pancreatic fluid to be well founded, an intelligible explanation is afforded. As fatty matters are found in the excrements, it must be inferred either that they have not undergone the usual decomposition and transformation, or that these fatty matters are secreted by the intestinal surface. In the former alternative, the reason must be that pancreatic fluid is deficient or diseased; and that the pancreas, not being capable of secreting healthy juice, must itself be in a diseased state. As to the latter alternative we have little information.

Another inference results from these experiments. It is not easy to say whether the alkali of the pancreatic fluid is exclusively destined for the conversion of fatty matters, or is likewise intended to saturate acidity of the alimentary mass, after it has passed from the stomach and been conveyed into the duodenum. Most physiologists, among others Prout, Tiedemann and Gmelin, and Bouchardat and Sandras, have generally taught, that the alimentary mass, after passing into the duodenum, is decidedly acid, and is there rendered alkaline. If this inference be well founded, then must it follow, that in cases in which there is much acidity, and where this extends through the alimentary canal, the pancreas must be in fault, and its secretion must be deficient, or at least less alkaline than it is in the normal state.

Dr Prout is of opinion, that the acid most likely to remain unneutralized, under these circumstances, is the lactic. The hydrochloric, he thinks, will be neutralized by the soda of the bile we suppose, and the pancreas. But as the lactic or any other destructible acid, which is either derived from the blood or from the food, or from both sources, leaves in the blood no equivalent of alkali, with which it may afterwards meet and be combined, it must be the cause of much evil within the duodenum and jejunum, and probably within the chyloferous vessels. The acid contents of the stomach do not in this case, as in health, on reaching the duodenum become neutral, but remain more or less acid; while this unnatural acidity not only produces great uneasiness in this part of the alimentary canal, but excites much disorder throughout the entire tube. Besides this, by being absorbed, it causes disorder in the vascular system, renders acid that blood which ought to be

\* Cases and Observations connected with Disease of the Pancreas and Duodenum. By Richard Bright, M. D., &c. *Medico-Chirurgical Transactions*, vol. xvii. p. 1. London, 1838.

Case of Jaundice with Discharge of Fatty Matter from the Bowels, &c. By E. A. Lloyd, Esq. *Ibid.* p. 57.

On the Discharge of Fatty Matters from the Alimentary Canal and Urinary Passages. By John Elliotson, M. D., &c. *Medico-Chirurgical Transactions*, vol. xviii. p. 67. London, 1833.

neutral or slightly alkaline; and hence may be the cause of those disorders, in which the perspiration becomes unusually acid, as rheumatism, gout, and similar maladies. To the presence indeed of lactic acid and other acids thus produced, Dr Prout ascribes the formation of struma and strumous diseases in early life, gout at the middle period, and rheumatism at all ages. It is easy to see that the non-neutralization of acid in the duodenum, whatever be the cause, must be a fertile source of disturbance and disorder in the assimilative functions.

Before concluding these observations, it may be proper to say that M. Bernard, as he has here examined the properties of pancreatic juice apart from bile, proposes to examine the effect of the union of these two fluids, and to ascertain in what manner their conjunction operates in promoting the process of digestion and assimilation.

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- ART. III.—1. *A Treatise on Cholera, containing the Author's Experience of the Epidemic known by that name, as it prevailed in the City of Moscow in Autumn 1830, and Winter 1831.* By JAMES KEIR, M. D., Professor of Pathology, Therapeutics, and Clinical Medicine in the Imperial Academy of Medicine and Surgery, Physician to the Charitable Institution of Count Sheremetieff, &c. &c., State Councillor, &c. Edinburgh, 1832. 8vo, pp. 138.
2. *Notes on the Epidemic Cholera.* By HARTLEY KENNEDY, M. D., &c. &c., late Physician-General, and President of the Medical Board, Bombay. Second edition, revised, 1846. Post 8vo, pp. 279.
3. *Fever Physiologically considered: Considerations on Yellow Fever, Typhus Fever, Plague, Cholera, and Sea-Scurvy; also the Questions of Contagion and the Quarantine Laws; with an Address to the Public on the Popular Treatment of Cholera.* By DAVID McCONNELL REED, Esq., Lic. Med., Member of the R. C. of Surgeons of Edinburgh, &c. &c. London, 1846. Post 8vo, pp. 262.
4. *Cholera, Dysentery, and Fever, Pathologically and Practically considered: Or the Nature, Causes, Connection, and Treatment of these Diseases in all their forms.* By CHARLES SEARLE, M. D., M. R. C. S. E., late of E. I. C., Madras Establishment. London, 1847. 12mo, pp. 128.
5. *Du Cholera; Moyens Preservatifs et Curatifs; ou Philosophie des Grandes Epidemies.* Par A. M. BUREAUD RIOF-

- FREY, D. M., de la Faculté de Paris, &c. Paris, Londres, 1847. 8vo, pp. 112.
- On Cholera; Prophylactic and Curative Measures for it; or the Philosophy of great Epidemics.* By A. M. BUREAUD RIOFFREY, D. M. of the Faculty of Paris. 8vo, Paris and Lond., 1847. Pp. 112.
6. *Researches on the Pathology and Treatment of the Asiatic or Algid Cholera.* By E. A. PARKES, M. D., London, Assistant Physician to University College Hospital. London, 1847. 8vo, pp. 250.
7. *British Cholera; its Nature and Causes considered in connection with Sanitary Improvement, and in connection with Asiatic Cholera.* By SPENCER THOMSON, M. D., L. R. C. S. E. et L. A. H. L. London, 1848. Post 8vo, pp. 110.
8. *The Nature and Treatment of the Epidemic or Asiatic Cholera; with Simple Directions for the Suppression and Prevention of the Disease.* By ROBERT VENABLES, A. M., M. B., Oxon. Inceptor Candidate, Royal Coll. of Physicians, London, &c. Fourth edition. London, 1848. 12mo, pp. 60.
9. *A Disquisition on Pestilential Cholera, being an attempt to explain its Phenomena, Nature, Cause, Prevention, and Treatment, by reference to an Extrinsic Fungous Origin.* By CHARLES COWDELL, M. B., M. R. C. S. London, 1848 (29th January). 8vo, pp. 210.
10. *A Discourse on the Asiatic Cholera, and its relations to some other Epidemics; including General and Special Rules for its Prevention and Treatment.* By HENRY STARR, M. D., Resident Practitioner at Leamington, and Senior Physician to the Warwick Dispensary. London, 1848. 8vo, pp. 95.
11. *Observations on Asiatic Cholera, during a Residence in St Petersburg in 1843, and on its Prevention and Cure; with an Account of the Sanitary Regulations proposed to be adopted against the Spreading of the Disease in this Country.* By ADAIR CRAWFORD, Esq., M. D., Member of the London R. Coll. of Physicians. London, 1848. 8vo, pp. 24.
12. *A Treatise on the Cause, Nature, Seat, and Treatment of Cholera.* By JAMES SHAW, C. E. L. L. London and Manchester, 1848. 8vo, pp. 39.
13. *Cholera. Practical Observations on a Successful Method of Treating Cholera.* By CHARLES PATERSON, M. D., Phy-

- sician to the Rathkeale Infirmary and Fever Hospital. (Dublin Medical Press, 20th Sept. 1848.) Dublin. 8vo, pp. 8.
14. *Diet and Cholera. Showing the Vital Importance of Wholesome Diet, and that its Impurities and Deficiencies are the Chief Cause of Cholera, with its premonitory Symptoms and Treatment; in a series of letters originally intended for insertion in the Times.* By WILLIAM BARNARD BODDY, Surgeon, late Medical Superintendent of the Cholera Hospital. London, 1848. 8vo, pp. 40.
  15. *Practical Observations on the prevailing Epidemic called Cholera, with Advice to the Heads of Families and others, as to the Precautionary and Primary Domestic Treatment to be used in warding off a threatened Attack of, and assisting those already afflicted by, the Epidemic.* By JAMES KEIR, M. D., Knt., Ex-Professor, Academician and Honorary Member of the Imperial Academy of Medicine and Surgery, Moscow, &c. &c. Part I., Edinburgh and London, 1848. 8vo, pp. 28.
  16. *Directions for the Management of Cholera in the Absence of Medical Advice.* By HENRY M'CORMAC, M. D., Member of the Belfast Sanitary Committee. Belfast, 1848. 12mo, pp. 12.
  17. *On the Treatment of Asiatic Cholera.* By ARCHIBALD BILLING, M. D., A. M., F. R. S., Author of First Principles of Medicine. London, 1848. 8vo, pp. 15.
  18. *Some New Views on Asiatic Cholera.* By ARTHUR LEARED, A. B., M. B. London and Dublin, 1848. 8vo, pp. 50.
  19. *Observations and Suggestions in regard to Cholera Asphyxia.* By CHARLES BELL, M. D., F. R. C. Ph. E., &c. L. Med. Gazette, 12th October 1848, pp. 3.
  20. *A Sketch of a Popular and Novel Treatment for Diarrhœa, Dysentery, and English and Asiatic Cholera; with Illustrative Cases of the Diseases.* By EDMUND SKIERS, M. D., &c. &c. of the Faculty of Paris, London, and Edinburgh. London, 1849. 8vo, pp. 91.
  21. *Two Lectures on Cholera and Intermittent Fever, addressed to the Members of the Medical Profession in Manchester, October 27 and November 3, 1848.* By CHARLES W. BELL, M. D., R. L. S., Physician to the Manchester Royal Infirmary, &c. &c. London, 1849. 12mo, pp. 101.
  22. *Questions and Observations in Hygiene.* By FRED. JAMES

- BROWN, M. D., London, &c., Assistant Surgeon, R. N. London, 1849. Post 8vo, pp. 64.
23. *Report on the Epidemic Cholera as it has appeared in the Territories subject to the Presidency of Fort St George. Drawn up by order of Government, under the Superintendence of the Medical Board.* By WILLIAM SCOTT, Surgeon and Secretary to the Board. Abridged from the Original Report printed at Madras in 1824. With Introductory Remarks by the Author. Edinburgh and London, 1849 (1st March). 8vo, pp. 212.
24. *The Contagion of Asiatic Cholera, deduced from its Recent Progress, its Early History, and its Pathological Correlations.* By EDWARD SPOONER, Esq., Member of the R. C. Surgeons, &c. &c. London and Worcester, 1849. 12mo, pp. 51.

THE apprehended approach of cholera, its advent, and its actual presence, all concurred to rouse the activity of the profession in offering advice as to the alleged causes of the disease, the means of prevention, and the method of cure. Of the works enumerated in the preceding list, a few are by authors who witnessed the first appearance of the disease in 1831 and 1832 in Europe; some are by those who beheld it in India before or since that time; and a number are by recent adventurers, who have had occasion to observe the disease either in Asia or in Europe.

Dr Keir appears, not only with his Treatise on the Cholera in Moscow in 1830 and 1831, but with a short code of prophylactic and therapeutic instructions. The former is distinguished by giving a clear and instructive account of the disease as it prevailed in Moscow, with much useful information on the means then employed as preventive and therapeutic. The second is short and practical. Dr Venables republishes his observations of 1832. Mr Scott publishes an abridgment of the Madras or Fort St George Report, reviewed in detail in our twenty-fourth volume in 1825.

Dr Hartley Kennedy publishes a second edition of his work, which appeared at Calcutta in 1826, in the same form exactly, in all respects, as that which attended its first appearance, without change, modification, or correction. The only addition is an appendix, containing several notes, of which the third gives an account of the mode of treatment of the bites of venomous serpents in India; the fourth, the history of an attack of cholera in a medical officer; the fifth, prescriptions for preparing the oils of spiders and earthworms; and the sixth, the account of a fatal attack in a medical officer, to illustrate contagious propagation.

The treatise by Dr Parkes is an excellent account of the dis-

case as it prevailed in various parts of India in 1843 and 1845, with judicious observations on the causes and modes of propagation, much accurate information on the morbid anatomy, and some useful remarks on treatment.

Dr Charles W. Bell of Manchester publishes two lectures, containing much new and interesting matter. Dr Bell is a nephew of the late Sir Charles Bell; and it is easy to recognise, in this small performance, the indications of the talent and intelligence, for which that family is distinguished.

The disquisition by Dr Cowdell is expressly written to render probable, if not to prove, that the cause of cholera depends on the operation of a poison issuing from the growth and germination of certain minute *fungi*, probably resembling the *torula cerevisiae*, or yeast fungus, but that the production of this vegetable is greatly favoured by certain electrical conditions of the atmosphere. The sporules of this fungus, he thinks, are taken into the system, we suppose, by inhalation by the atmosphere, and, being mixed with the blood, give rise to the disease.

It is impossible to notice each of the performances, the titles of which are placed in the foregoing list. All that we can do is to take a general view of the contents of the whole. We shall advert to the alleged causes, some points in the pathology, and the most important rules regarding treatment, whether prophylactic or therapeutic.

ETIOLOGY.—In several respects, progress has been made, if not change and revolution, in the doctrines on the alleged causes of cholera. It is true that the patrons of the doctrine of contagion have remained firm to their principles; they have undergone no conversion in the lapse of seventeen years; and they and their adherents are as thoroughly convinced of the truth of the doctrine that cholera is an imported article, and is conveyed from one part of Europe to another, at the present time, as they were in the years 1831 and 1832, when quarantines, sanitary cordons, and segregation, were the orthodox facts of the day. This is not wonderful. No one willingly changes his opinions; because any change implies that he was in the wrong, or that he is inconsistent. In this respect, therefore, the advocates of the doctrine that cholera is a contagious disease, contagious in origin, and contagious in the mode of propagation, are quite consistent and faithful to their principles.

On the other hand, however, it must be allowed that the defenders of the opposite doctrine are more numerous, and that the doctrine that cholera does not depend upon contagion, is neither treated with the same degree of hostility and contempt which was accorded to it, nor does it number so small an amount of believers. That doctrine has, indeed, at length triumphed over a large and strong amount of opposition, and bids fair to command, if not the

majority, at least a very respectable minority, among those who take interest in questions of this nature. It is further to be observed, that the doctrine of non-contagion, not only as to cholera, but as to yellow fever, plague, and dysentery, has received a sort of public sanction by the reports of the Sanitary Commission. The statements made in these reports contain, indeed, nothing new; not one new fact, not one new doctrine to the readers of this Journal. We have long and repeatedly taught what we believe to be the correct views of the origin and mode of propagation of yellow fever; we have shown how useless are quarantines in stopping the progress of that distemper, and how necessary it is to look to other causes than contagion. As to cholera, this Journal stood almost alone in 1832; and though we could not exclude from its pages the views of those who adopted the doctrine of contagion, sufficient evidence was adduced in various papers to prove, that the disease was not only not contagious, but that the doctrine of contagion was contradicted by all the best authenticated facts. It is not unimportant to observe that the views given in these papers have been, in the course of the interval, tacitly adopted, it may be said, without acknowledgment, by various authorities, who are now adduced as the sole labourers in this field of research. In regard to this we feel quite indifferent. It is gratifying to observe that the doctrines promulgated in this Journal, both as to the disease not being new, and not being a contagious distemper, have received in this manner proofs and confirmations which tend only to corroborate their accuracy.

The same may be said regarding the etiology of plague and dysentery, both of which, we have repeatedly endeavoured to show, depend upon local physical causes, and certain atmospheric conditions.

It is therefore not matter of surprise to us, that the same reasonings have been applied to cholera as to all similar diseases; and that, though many still believe, that the disease originates in, and is propagated by contagion, yet the numbers are daily increasing of those who believe that it depends, both for origin and propagation, on local causes and certain atmospherical conditions.

It is unnecessary to go over all the statements, now somewhat stale, of this disease having commenced in Jessore in 1817, although we observe several persons really believe, that this is a matter of fact. If they would take the trouble to read on this point, what has been published in this Journal during the last twenty years, and repeated from its pages in multiplied works, they would at once, if not void of all candour, admit that this statement is altogether void of foundation. No one denies that cholera, in a severe form, arose and prevailed at Jessore in 1817; and that this prevalence was the apparent beginning of a series of epidemics, which



appeared in different parts of India. But it is impossible to maintain, that cholera did not prevail in India long before this time,—and equally impossible to deny, that, between the various places in which it afterwards prevailed, it was quite impracticable to trace any connection or communication. It is on this account that most of the Indian observers ascribed the origin of the disease either to malaria, that is, telluric miasma, or to some atmospheric peculiarities and conditions connected with a certain state of electrical phenomena. This is the view taken by Dr Searle, Dr Parkes, Dr Bell, and many more. Dr Hartley Kennedy, on the other hand, adopts the doctrine of contagion in all its force, and views the disease through no other light except that of a contagious principle. The facts adduced by Dr Searle are particularly conclusive, as he witnessed the disease not only in India, but in Europe, at Warsaw, in 1831. In the latter situation, while he distinctly traces the disease to the influence of an epidemic constitution, to some poison moving along the surface of the earth, and to dietetic errors and modes of living, he could observe no proofs of anything like contagion, (pp. 34, 35, and 110). Extreme humidity of the atmosphere also, a condition attended with a low state of the barometer, and diminished electrical tension of the air, he considers as a powerful occasional cause; and when to these are added states in which the human body is placed favourable to lowering its vital powers, as fatigue, exhaustion, inanition, and low spirits, then cholera, he maintains, is most likely to appear and commit great havoc. Such, he recalls to recollection, were the circumstances in which was placed the 86th regiment at Kurrachee in June 1846, which, from circumstances of this kind, namely, fatigue and privation succeeding to a long march, was attacked first, and lost more than double the number of men of any other corps.

The first case appeared in the regiment on the 11th of June 1846; and, by the 30th of June, 399 cases had been admitted in the course of sixteen days; and, of this number, 235 died. In other two regiments, namely, the 60th Rifles and the 1st Bombay Fusiliers, at this station, cholera appeared about the same time with great violence. But, in the 86th regiment, the number of cases was three times as great as in the 60th, and rather more than twice as many as in the Fusiliers. The proportion of fatal cases to admissions was nearly the same in all the three regiments.

Mr William Scott rather states in his report the facts and reasons for and against the truth of the doctrine of contagion, than takes any decided side. But, in the introductory remarks which precede the present abridgment, he is something inclined to the side of contagion. It is true that he allows that at present the same doubt, the same uncertainty, the same indecision, as existed

when the disease became a subject of general interest, continue to prevail. He allows that the disease may arise spontaneously, that is, in some manner, where terrestrial miasmata prevail, and peculiar atmospheric conditions exist; but he maintains, that this admission does not preclude the operation of contagion as an adjunct, after the disease is established. He admits the inefficiency of quarantine regulations and sanatory lines of enclosure, as well as seclusion and non-intercourse, in preventing the introduction of the disease. Yet he speaks as if it were and could be imported. He thinks it resembles in many respects influenza; and, in so thinking, must be implied the idea, that it is both atmospherical and non-contagious. But he concludes that, whether its cause be an animal poison or an atmospheric poison, is also equally involved in obscurity and mystery.

Dr Parkes, who saw the disease at Moulmein in the Tenasserim provinces, between September 1842 and July 1843, and at Madras, in the months of June, July, and August 1845, never observed in the whole course of the two epidemics any indication of contagion. With most Indian writers, his evidence is on the negative side. During the prevalence of the disease in Tenasserim for months, he never heard human intercourse represented to be the means of introducing it into any place. The Burmese and Tailiens do not consider it contagious, and do not avoid their relatives when sick, or omit the customary ceremonies when they die of the disorder. No precautions of non-intercourse were observed at Moulmein; and those who were in the freest intercourse with the sick were not in consequence of that intercourse attacked. Neither in the military hospital in 1843 and 1845, nor in the jail hospital, where Dr Parkes was engaged in making morbid inspections, was it observed, that the hospital attendants, however constant and free was their intercourse with the sick, were on that account more frequently or more numerous attacked. In the jail hospital, indeed, none of the medical officers or attendants were attacked.

What, then, is the cause of the appearance of the disease in any particular locality, and what are the circumstances on which its propagation depends? These questions it is not easy to answer. Dr Parkes positively states, that, neither at Moulmein nor at Madras, were any unusual or peculiar states of the atmosphere perceptible. The rains, indeed, were later in appearing at Madras in 1845 than usual; but this retardation of a meteorological visitation was not greater than was observed in years in which cholera did not appear. Suddenly, he infers, into each place, enters some new agent, which finds its conditions of development and energetic operation not in any unusual atmospheric vicissitudes, but in ordinary local and physical circumstances.

At Moulmein, the disease appeared at first in the habitations nearest to the river, and affected, almost exclusively, the houses on the river side of the street; and the only reason for this preference is, that this district is the most depressed, the dampest, and the most thickly peopled quarter. Here the distemper prevailed for months before it invaded another quarter of the town. It attacked not the residents on the high grounds; and when it extended from the points which it originally seized, the conditions under which this extension took place, were to be recognised in the occurrence of meteorological phenomena previously wanting.

For many months, the disease was confined almost entirely to the houses situate on or over the river, it is stated, by which we understand close to the river, and chiefly to the south end of the town. After prevailing in this locality for months, the disease spread gradually over the whole town, which extends for four miles along the banks of the river Salwyn. The population of Moulmein was at the time (1843) estimated at 25,565 persons. The number of attacks is not stated. But between the 11th October 1842, when the disease commenced, and the 21st of March 1843, at least 1377 persons were destroyed. The author appears to think, that this number is greatly below the fact, as many Burmans died whose deaths were not reported.

A similar course was observed on the appearance of the disease at Madras in 1845. It was stated that cholera was prevailing at a station ninety miles distant. In the course of a few days, it appeared at Madras itself. The atmospheric phenomena were not very different from those of years during which no case of cholera occurred. But a certain degree of moisture was present in the atmosphere, and a wind was blowing directly from the station, in which the disease had previously been prevailing. The visitation caused great mortality in the Black Town, crowded with Hindoos, and in the quarter of the Moslemin congregated around the palace of the Nuwaub, both places dense with a dirty and offensive population. The distemper was less severe in the fort, in which English soldiers were quartered, and which was accordingly kept tolerably clean. It was not seen at all in the houses of the English residents, scattered for miles along the chief roads and the sea coast. In both cases, the author thinks there is evidence of the existence of a poison exhaled from unknown sources, moving along the surface of the ground, through the atmosphere, entering a town, and producing its effects more or less rapidly and severely, according as it meets with humidity, effluvia of animal and vegetable matters, and perhaps other terrestrial exhalations, which are therefore inferred to form certain conditions favourable to the development and extension of the agent.

Again, it has been repeatedly observed, that the disease has

appeared to move from one place to another, not on the wings of the wind, as is usually said, but against the atmospherical current ; and this circumstance the contagionists have repeatedly adduced with great confidence, as a proof of the contagious nature of the distemper. On the other hand, instances have occurred in which a vessel approaching the coast, or working her way up the Ganges, has been struck, as it were, as instantaneously, as if with a gust of wind, or a flash of lightning, and thirty or forty of her crew have been speedily attacked by the disease.

Much has of late years been said on the effects of the exhalations from decomposing animal and vegetable matters, or the contents of uncovered drains and ditches, obstructed drains and sewers, conduits of foul water for the purposes of irrigation, stagnant ponds, slaughter-houses, obstructed water-closets and necessaries, and, in short, all situations in which animal matters, and especially excrementitious matters, have been allowed to accumulate,—as generating causes of cholera, and similar diseases. The circumstance that most forcibly directed attention to the operation of this class of agents, was the pernicious effects of the air which escaped from the contents of a foul obstructed cesspool at Clapham in August 1829. The contents of this receptacle of filth were thrown over a garden adjacent to the play-ground of a boys' school. Among twenty-two boys, twenty were attacked by vomiting, purging, and symptoms, it is said, similar to those of cholera ; by these symptoms two died in the course of two days.\*

Since the date of the occurrence now mentioned, a vigilant observation has been maintained on all cases of cholera, fever, and diarrhoea, in connection with drains, obstructed or open, necessaries in bad order, sewerage in general, lay-stalls, slaughter-houses, and all sources of nuisance ; and in London especially, so energetically has this system of observation been carried on, that it seems now to be established as a certain fact, that, between these nuisances and cholera, diarrhoea, and fever, there is an undoubted and necessary connection. One excellent effect of all this agitation and the consequent cleaning, was that many nuisances were abated and extinguished previous to the recent advent of the disease ; and in several instances, attempts to improve the drainage of the metropolis were made, with variable degrees of success.

To the same causes is the prevalence and great severity of the disease in Paris, in the visitation not yet finished, ascribed. M. Bureaud de Rioffrey, though believing in contagion, tells the Parisians without hesitation, that the principle cause of the violence and mortality of cholera in 1832 is the state of their drains, their sewers, and their latrines. Of the last he speaks with absolute horror, as utterly disgraceful to any nation or com-

\* Medical Gazette, Vol. iv. p. 375. London, 1829.

munity, and greatly more so to the polished inhabitants of the metropolis of France.

"If Paris," he says, "has been the town in Europe most severely handled by cholera, I hesitate not to ascribe this result to the repulsive and shameful condition of the latrines in almost all the houses." In order to protect Paris from such visitations, he thinks it indispensable that a complete revolution in the manners of its inhabitants in this matter will require to be effected. Unfortunate city, it may be said; how many different sorts of revolutions must it pass through before it can be thoroughly right! It still suffers under the epidemic; and it is to be feared that another visitation of a different kind is awaiting.

It would be idle to deny, that obstructed drains, especially if uncovered; necessities, without adequate provision for constant removal of contents; slaughterhouses; laystalls, and all places in which animal and vegetable matters are allowed to accumulate,—are very great nuisances, very hurtful to the neighbouring air, and to the health of the human beings compelled to breathe that air; and it is very probable, that air so contaminated may have produced cholera, diarrhoea, dysentery, and various other distempers. All localities in which the nuisances now mentioned are suffered to exist, abound in the formation and evolution of hydrosulphurous acid gas; and we have seen what pernicious consequences are ascribed by Mr Webb to the presence of this gas in certain regions in India. The effects of all these filthy nuisances near human habitations have been long known, and were pointed out many years ago in this Journal as to Newcastle, Newburn, Fisherrow, Dantzic, and various other places. It is, nevertheless, impossible to recognize in these sources of foul and offensive vapours the constant and uniform causes of cholera. Hydrosulphurous acid gas, when concentrated and abundant, extinguishes life at once. But it is incessantly mixed with the atmosphere, especially if moist, and in the great majority of instances it is rendered feeble and inert, and comparatively harmless. It is only when the presence of this agent is associated with certain states of the atmosphere, that it appears then to act in some sort as a cause of the disease. But how many facts and arguments does the whole history of cholera furnish against the inference, that it is the constant and essential cause of the disease? Were there any truth in the representation, then neither London nor Paris, nor Calcutta in particular, could ever be free from cholera. It is enough to observe, that in numerous places there are sources of foul mephitic vapours; and yet in these, cholera has either not prevailed at all, or has not prevailed in a greater degree than in other places without such sources of nuisance.

Dr Charles W. Bell, who has seen the disease in Persia as

well as in this country, regards it as the offspring of malaria, or telluric miasma, which being imbibed by the blood, alters that liquid, and operates, through the medium of the sympathetic system, upon the economy at large. He further allows, however, that at certain periods the atmosphere becomes modified in some unknown manner, so as to produce, according to circumstances, ague, remittent fever, cholera, or dysentery. The phenomena of cholera he ascribes to a peculiar atmosphere, which he believes pervades chiefly the lowest levels of a tract of country. But he mentions, as a remarkable exception to this fact, which has been already noticed by almost all observers, that one of the spots pointed out to him as most fatal in Persia, was elevated 10,000 feet above the level of the sea, another 8,000, and another 5,000 feet above that level. In that country, though as in others the disease follows the course of rivers, it has been uniformly most fatal in the neighbourhood of the extinct volcanoes which crown the great range of elevated country extending from Ararat to Khorasan. On this point several curious facts are mentioned. Thus the first advance which cholera made upon Persia in 1828 and 1829 was preceded by multiplied earthquakes in 1827-8, which extended for some hundred miles round the extinct volcano Demawund. A period of repose from subterranean commotion then followed till 1840; when a violent earthquake shook down a portion of Mount Ararat, and continued to agitate the country to the south-east of that mountain till December 1843. Then for the first time, within the limits of the historical period, a new volcano burst out near Shoomacha, thirty miles to the west of the Caspian Sea, and probably one hundred miles to the north of Ararat. In 1841-42-43 the whole tract of country between the mouths of the Indus and the Caucasus had been ravaged by unwonted dysentery, pestilential fever, and in some places cholera; but after this eruption, earthquake ceased, and the country became comparatively healthy till 1846, when cholera again broke out with virulence in the same tract. Since that time the main line of the advance of cholera has been in the direction of the volcanoes of Iceland. It would be important to ascertain, whether in the neighbourhood of these volcanoes, or along this line, is extricated any particular mephitic vapour which might exercise some influence on the production of the disease.

Dr Bell allows that the facts now specified do not explain the rise and propagation of cholera in other countries; but he thinks that it may become among philosophers a question of some interest to determine whether there can be in the course of this line, extending from Cutch and Central Asia over the Elboorz and Caucasian mountains towards the great volcanic chimneys of the northern hemisphere, any state of the crust of the earth which is

adequate to cause these sudden and violent outbreaks of epidemic and apparently poisonous sickness.

Another speculation on the cause of cholera is advanced by Dr Cowdell. It is impossible to follow the author in the list of facts and arguments which he adduces in support of what he denominates the origin of cholera, from the absorption of poisonous fungi. The hypothesis is shortly the following. In the lactic and viscous fermentation are appearances and products which much resemble what is found after pestilential cholera, in the blood, the secretions, and some of the tissues; the state of the blood in cholera may be regarded as representing the first stage of mortification of the solids; several *fungi* produce poisonous effects closely resembling the symptoms of pestilential cholera; from the abundant mixture of one of these products in food, results an epidemic disorder affecting the inhabitants of whole countries on the continent; that in such epidemic gangrene and wasting of the solids, are the pathognomonic characters of one of its forms, nearly resembling the disease arising from the use of poisonous sausages; that in the ergotism fungus on the one hand, and on the noxious sausage on the other, is a remarkable abundance of oily matter, which is further a characteristic of the blood, in which the organisms are supposed to be generated; and that the *sporidia* of such fungi may have been generated within Jessore and in Kurachee, and in other situations in which cholera has anew appeared.

The hypothesis now stated it is unnecessary to examine minutely. The chief reason for giving this brief mention of it at present is, that it may be made known, and that it may be subjected to the test of observation. Against it, various objections might be stated; but in its favour are also other circumstances; and it is only by observation that its accordance with facts can be ascertained.

We shall have occasion to show that a similar hypothesis on the origin of endemic fever has been proposed by Dr Mitchell, an intelligent American physician, and illustrated with great learning and force of reasoning.\*

**MORBID ANATOMY AND PATHOLOGY.**—By far the most important information on these heads is communicated in the treatise by Dr Parkes.

Much blood was found accumulated within the veins of the *dura mater* and *pia mater*; and vessels of the brain; and serous fluid was effused.

Blood was accumulated in the large vessels of the lungs. But the small vessels were usually contracted and empty. The air-vessels were collapsed. In some cases blood was found in the

\* On the Cryptogamous Origin of Malarious and Epidemic Fevers. By J. K. Mitchell, A. M., M. D., Professor of the Practice of Medicine in the Jefferson Medical College of Philadelphia. Philadelphia, 1849. 12mo, pp. 137.

minute structure, with a dark colour of the lung; and some frothy serum which must have been in the minute bronchial tubes.

The right side of the heart and the pulmonary arteries were distended by blood. In the left side and aorta the quantity was small. In 12 among 34 cases large polypiform or fibrinous clots were contained in the right cavities. In 7 cases were small loose dark-coloured clots on the right side only. In 7 cases no clots were found at all; but the blood from the right side coagulated more or less firmly on removal. In 8 cases were no clots in the heart or elsewhere; and the blood remained uncoagulated. The blood was generally dark coloured. Marks of the deficiency of fibrine were manifest. Albumen and salts were present in undetermined quantity.

Blood was collected in the large branches of the *vena portæ* and hepatic vein; the gall-bladder was moderately full; the bile thick and viscid.

Little change was observed in the pancreas, spleen, and kidneys. The spleen and kidneys were in a small proportion of cases found congested with dark-coloured blood.

The most important circumstance in the state of the abdominal viscera generally was, that all their veins were more or less loaded by dark-coloured blood.

The intestinal mucous membrane was in a considerable proportion of cases reddened. The agminated and isolated glands were in the majority of cases enlarged. But no ulceration or sloughing of the agminated glands was observed in any case.

Disease of long standing was less frequently observed in the several internal organs, as the lungs, liver, and kidneys, than in the cases observed in Europe.

The alimentary canal either always or most commonly contained whitish-grey, rice-water, or gruelly-looking fluid, distinguishable into a thin serous or watery portion, and a thick clotted or curdly portion.

This fluid, the author infers, consists of part of the water and salts of the blood, mixed with a proteine constituent, in other words, fibrinous and albuminous matter.

It remains also to be observed in regard to the use of the term *algid*, employed by Dr Parkes after the example of Chomel, that though this term may have been applied to cholera first by Chomel, it cannot be viewed in any other light than as adopted from Galen among the ancients, and Ettmuller, Van Helmont, De le Boe, and Torti, among the moderns. Galen mentions the *amphimerina phricodes* as a form of telluric fever with long-continued cold. *Febris algida*, or chill fever, is a term not uncommon among various modern authors, as Ettmuller, Van Helmont, and De le Boe; and Torti uses the same denomination, *febris algida*.



to designate one of the severe fevers of modern Italy, with intense and long-continued cold stage; and from him and others Sauvages has adopted the term, as expressive of that form of remittent in which the surface is cold, and the pulse gone (*asphyctos*), and in which the patient is suddenly destroyed.\*

**TREATMENT, PROPHYLACTIC AND THERAPEUTIC.**—If we distinguish the treatment into preventive or prophylactic, and curative or therapeutic, it will be requisite, so far as the rules for the accomplishment of the former object are to be carried into effect, to have a steady eye upon the exciting causes by which the disease is known or believed to be produced. Thus, though it may be impossible to apply remedies to counteract the state of the atmosphere, and not very easy to neutralize and counteract the unhealthy state of the terrestrial surface, yet it is practicable to remove all those causes, which may tend to deteriorate the atmosphere, to observe local cleanliness and dryness, and in some instances to withdraw the inhabitants from localities manifestly deleterious. Drains and ditches may be cleaned out; receptacles of filth may be removed; and from habitations on the borders of canals, ponds, or slowly-moving rivers, the inhabitants may be removed.

The part of prophylactic medicine, however, which is most manageable and practicable, is what relates to individuals. They may observe personal cleanliness; they may avoid errors in diet; they may avoid the use of improper and hurtful articles of food.

Upon these points all the present authors are so full,—indeed, all writers are so particular,—that it is quite superfluous to say one word on the subject. The doctrines given by Dr Keir, Dr Searle, Dr Adair Crawford, Mr Boddy, Dr Spencer Thomson, and indeed many others, show that on these points there is no lack of directions.

In one circumstance it must be agreeable to observe a great improvement in these directions, and a return to common sense, after the panic about articles of food in 1831 and 1832. At that time succulent and fresh vegetable products of all kinds were denounced, and condemned in the most unqualified manner; and nothing was permitted but bread, rice, beef-steak, and similar solid viands. Now for all this severe and sweeping condemnation of the use of fresh vegetables and ripe fruits, there was either no reason at all, or the reasons were very insufficient. There was no reason to believe that the moderate use of good vegetable articles of food, if properly dressed, could be in the least degree hurtful; and there was every reason to believe that they would be beneficial. Experience had never shown that their use was productive

\* *Nosologia Methodica*. Class ii., Febres. Vol. i. pp. 325, 326. Amstelodami 1768. 4to.

of cholera; and it had shown that their use was actually curative in dysentery and diarrhoea. Here, however, abuse and improper applications were confounded with moderate and seasonable use; and many thousands of persons were for months actually terrified out of the use of articles of food to which they had been accustomed, and which had been agreeable,—all for no other reason but because boards of health and cholera doctors declared that the fruits of the earth were then equivalent to poison. From this strange hallucination this part of the profession have at length in some degree recovered. They are not quite sure of the absolute innocence of turnips and spinach, carrots, green peas, or even oranges, but they cautiously hint, that these articles may not be so noxious in 1849, as in 1832 they were confidently represented to be. The members of the Metropolitan Board of Health express themselves in guarded language, and considering the decided bulletins which they issued in 1831 and 1832, caution is at least decent and prudent. Conversions should neither be sudden nor violent. All the authorities agree in condemning raw vegetables, sour fruits, that is unripe fruits, the use of parsley, fat oleaginous meats, sour beer, and sour wine, on the ground, not that they produce cholera, but that they disorder the stomach and bowels, and hence, may predispose to the formation of the disease. This is the whole length that it is prudent or necessary to go. In all other respects the moderate use of the ordinary articles of food is recommended to be continued as in ordinary circumstances. When to these dietetic rules are added, the avoidance of cold in all forms, of excessive fatigue, of excesses in eating and drinking, and due regulation of the several functions, all that is within human power, as to the prevention of the appearance or approach of the disease, is stated.

When the disease has actually established itself, either in a community or in individuals, the treatment is distinguished into three sorts; one suited to the early or preliminary stage, one suited to the established stage, and one suited to the stage of collapse.

The great object in the preliminary or incipient stage, all experience shows, is to check the premonitory diarrhoea. It is true that it is impossible, in all cases of diarrhoea, to say whether any given case, or how many of a set of cases, are to proceed to the established and collapsed stages. It nevertheless is certain, that the great point is to check the diarrhoea; and the practical rule is, during all periods, when any circumstances indicate the presence and influence of an epidemic state of the atmosphere, that no time should be lost in accomplishing this indication.

It is further of little moment how this object is accomplished, providing it be accomplished. The simplest remedies appear to be the best. A little chalk mixture, the opium and chalk pow-

der, Dover's Powder, opium alone, laudanum alone, rhubarb and magnesia, have all been found adequate, with attention to diet and warmth, to arrest the progress of the disease in this stage.

In the established or confirmed stage of the disease, the transition from the preliminary one is often so gradual, yet sudden and rapid, that the utmost vigilance in observation is required, and the greatest promptitude in the administration of measures may be inadequate to prevent the approach of the ensuing or third stage, namely, that of collapse. This, nevertheless, is the great object; and, upon error or indecision, or inefficiency at this crisis, depends, so far as human means are concerned, the termination of the attack in convalescence or in death. The remedies to be given at this period are multiplied, if we look to directions; the modes of treatment numerous. Calomel and opium, blood-letting, acetate of lead and opium, the application of external warmth, sinapisms, emetics, especially the salt water emetic, the mustard infusion emetic, blue pill, strychnia, brandy and water, the Corbyn draught, or sulphuric ether and laudanum, chloroform, clysters of laudanum, of salt, are only a few of the ten thousand means which have been at this time suggested as efficient in retarding the downward progress of the disease, and averting the fatal termination. It is unnecessary in this place, and would be altogether unseasonable, to consider the merits of even any one of the numerous methods of treatment proposed by different authors. Any one of the tracts and treatises before us is sufficient to answer all useful purposes. But the most convenient, perhaps, may be the tracts of Mr Searle and Dr Billing.

The latter is by far the most original, the boldest, and the most decided of the dealers with previous and apparently-established methods. The directions of the Board of Health he treats with everything but deference and respect; and, after reading his tract, the conclusion is, that members of boards of health are among the most imbecile and inefficient of human beings, always excepting lords of the treasury, and old ladies that attend the Ebenezer chapel. Sal volatile, recommended by the manifesto of the Board of Health, is inefficient; the hot water to be given with it is positively injurious. The hot brandy and water is also injurious; and, if the patient do not die in the cold stage, the quantity of brandy in his inside will add to the fever, when he arrives at the warm stage.

Dr Billing regards cholera as a species of miasmatic fever, resembling ague in having hot and cold stages. The chief difficulty which must occur to this view is, that the order of these two stages is different in ague and in cholera. In the former the cold stage comes first, and is succeeded by the hot stage. In cholera the first part of the disease is a sort of warm stage, passing rapidly to

the state of cold. It must be allowed, nevertheless, that between the two are many common points of agreement and resemblance. Whether, however, this similitude be correct or not, the author applies it in the treatment of the disease, and advises that cholera should be considered merely as a severe form of miasmatic fever. The cold stage of cholera is like the cold fit of ague, the preliminary attack of small-pox, or inflammation tending to suppuration in an internal organ, as the lungs or the liver. The practitioner, therefore, must not give brandy and water. He must exhibit an emetic, and a mixture of Epsom salts and tartar emetic; and tablespoonful or teaspoonful doses of this, according to age and strength, will restore heat better and sooner, and more safely, than all the usual means of applying external heat. Cold water should be drank freely. Then calomel may be given in five grain doses to act on the liver. After this, bisulphate of quinine is a good remedy. Antimony, in short, and saline medicines, are the great curative agents.

All this may be very proper and rational. But it is not unreasonable to state, that in one of the early Indian epidemic visitations of cholera, to the best of our recollection, some time before the close of the last century, it is said, on authority supposed to be good, that tartar-emetic was given rather on an extended scale; and it was apprehended, that it had not only done no good, but it was to be feared increased the mortality. It must be allowed nevertheless, that it is so difficult to ascertain what diminishes or what increases the mortality in a disease like cholera, that, supposing that this had been the fact that antimony was administered in any given epidemic, it would by no means follow, that this mineral was the cause of the mortality. We believe it is quite as difficult to increase as to diminish the mortality of epidemic diseases of this nature; and probably their symptoms are more likely to be aggravated by much treatment than by no treatment. But, at all events, the method inculcated by Dr Billing, which, he says, has been adopted with marked success by friends in London, Paris, and other places, deserves a fair trial.

In one detail of treatment he is, without controversy, right. He allows the patients to take cold water with the utmost freedom. Whoever has seen patients either in the established stage or the collapse stage of the disease, must admit, that to refuse these persons cold water on the ground of its aggravating their symptoms and increasing the vomiting and purging, is the most cruel as well as useless and hurtful proceeding that can be imagined. Suppose it given, and it is rejected by vomiting, this is only what it is often desirable to effect. If it be not rejected, it certainly does not aggravate the symptoms and increase the dis-

charges. Much more frequently it tends to re-establish healthy reaction, and to recal the circulation to its normal channels.

Dominico Panaroli, a Roman physician, who flourished in the seventeenth century, records for the edification of his readers the fact, that, in the year 1649, exactly two hundred years ago, an infinite number of patients,—these are his very words,—were burnt and put to death through the cruelty of certain physicians, who would not permit them to drink cold water.\* He adds that, upon inspecting the bodies of the dead, the stomach, heart, lungs, and other internal organs were found, as it were, burnt up. By following in his own practice a cooling regimen, and allowing patients to take barley water and cold water freely, and thus promoting the secretions of the kidneys, the skin, and the bowels, he succeeded in restoring many to health at a time when funerals were most numerous, and whole streets were filled with grief and mourning.†

Now, making allowance for every exaggeration, it is easy to understand, that, whether the mortality was actually increased or not by withholding cooling drink, the sufferings of the patients must have been greatly aggravated, and the course of the fever must have been prolonged. The fact now mentioned we give to Dr Billing the more readily, that the Roman distemper of 1649 was the common summer remittent of Rome, from which cholera, as a disease similar to ague and remittent, cannot be widely different.

It is remarkable how many testimonies are given against the employment of external warmth, whether in the form of hot bath or dry heat. Certainly we must say we never saw much good result from the use of external heat in any form. The warm and the hot bath are uniformly condemned. We believe it would be safer, if any external applications are employed, to have recourse to the cold affusion, the cold sheet, or any other method of applying cold, than to use applications which seem positively injurious. Dr Bell states that one friend writes from Tehran, that the practice of immersing the patient to the neck in cold water till reaction took place, was very successful. Another mentions that he cured patients by frictions with snow at Erzeroum, where cholera occurred in the winter season. Another fact of the same tendency is the following. Dr Bell is well acquainted with two Persian gentlemen who were laid out for dead during the

\* Sunt quidam, qui eandem fere legen in omnibus febri erantibus servant, nimirum, ut caveant maximopere a frigida potu; quam ob causam anno 1649 infinitus propemodum aegrorum numerus a vivis sublatus est; cum tamen febres essent continuæ; et qui cum siti maxima torquerentur exusti obibant.

† *Jatrologismorum Seu Medicinalium Observationum Pentecostae Quinque. Opus Dominici Panaroli Romani Philosophi et Medici in alma Urbis Archilycaeo Medicinæ Professoris. Hanoviæ, 1654. 4to. Pentecoste Quarta. Obs. viii. p. 214.*

cholera of 1829. The bodies were carried out to the open court-yard in a winter night, in order to be washed, previous to interment. To obtain the water required for this purpose the ice had to be broken; and after the operation of washing in this cold water, both bodies became warm, and reaction taking place, they finally recovered.

Such facts show that cold, applied to the surface, is much more likely to be beneficial in this stage than heat.

It is not wonderful that Dr Billing speaks of the instructions of the Board of Health with small respect; and indeed he asks more than once whether the parties who gave such instructions could have seen or treated one case of cholera. One of their cautions will not a little amuse practical observers. They dissuade from the use of castor oil as a dangerous remedy, and one apt to induce the disease.\* Independent of what every experienced physician observed as to the utility of this agent, during the epidemic of 1832, 1833, it is remarkable that in favour, not only of its harmlessness, but of its positively beneficial effects, there are two strong and decided Indian testimonies. In an interesting memoir on the diseases prevalent among the natives of Hindostan, published in the twenty-fourth volume of this Journal, Mr John Henderson, after some judicious observations on the actual merits of calomel, laudanum, and other medicines, states, that, by giving this medicine first in one large dose, and then in repeated small doses, and confining the patient to the horizontal position, he never lost one case. Laudanum and other accessory remedies he regards as next to useless.† Secondly, Mr Twining in his essay on cholera states, when speaking of the alleged hurtful effects of certain purgative medicines, that castor oil is of the whole tribe the only one, which never was followed by bad effects, and which he employed on all occasions with the greatest advantage. The Commissioners must either have been ignorant of these facts, or determined to disregard them, when they issued this valuable admonition.

Cholera, as an epidemic, is, it is to be hoped, in this country, nearly over. Yet no one can tell how soon a visitation may recur, and what places, which have hitherto escaped, may yet be affected by its ravages. In Ireland it appears to be only commencing; and in one or two places in this country,—for instance, Dundee,—its first attacks have taken place only recently. It may therefore be dreaded that there is yet to be room for observation and experience.

The Court of Directors of the Honourable East India Com-

\* "Avoid purgative medicines, particularly castor oil, Seidlitz powders, and salts."—*Precautions and Instructions from Commissioners of Health.*

† *Observations on the Diseases prevalent among the Natives of Hindostan.* By John Henderson, Assistant Surgeon, H. E. I. C. Service, Bengal. Edinburgh Med. and Surg. Journal, Vol. xxiv. pp. 32 and 41. Edin. 1825.

pany have authorised a copy of the treatise of Dr Searle to be given, at the expense of the company, to each medical officer in their service.

ART. IV.—1. *A Treatise on the Cure of Ulcers by Fumigation; in which a Rational Treatment is deduced from the Physiology of Ulceration, and Proofs afforded that the New Method produces more speedy, certain, and permanent Effects than any other in general use; with an Analysis of the Modes of Cure hitherto employed, and an Exposition of the Decided Advantages possessed by the New Treatment.* By GEORGE ALFRED WALKER, Surgeon, Member of several Learned and Scientific Societies, &c. &c. London, 1847. 8vo, pp. 112.

2. *On the Treatment of Ulcers on the Leg without Confinement; with an Inquiry into the Best Mode of Effecting the Permanent Cure of Varicose Veins.* By HENRY T. CHAPMAN, F. R. S. C., late Surgeon to the St George's and St James's Dispensary, &c., formerly House Surgeon to St Bartholomew's Hospital. London, 1848. Post 8vo, pp. 156.

3. *Lectures on the Causes and Treatment of Ulcers of the Lower Extremity. Delivered at the London Hospital during the Summer of 1848.* By GEORGE CRITCHETT, Esq., F. R. C. S., Surgeon to the Royal London Ophthalmic Hospital, Assistant Surgeon to the London Hospital, and Lecturer on Practical Anatomy, &c. London, 1849. 8vo, pp. 121.

FEW subjects in practical surgery have proved so puzzling as that of the treatment of ulcers on the lower extremity; and, accordingly, few subjects of the same apparent importance have given rise to so many treatises and essays, professing to improve and render efficient that treatment. The method proposed in 1797 by Mr Baynton, by means of tight strapping and bandaging, was long believed to be a real improvement, and to be most generally effectual in accomplishing cicatrization. But, whether in consequence of its being abused and misapplied, or that the tight strapping was carried too far, it is certain that the number of cases of atrophied and otherwise deformed legs which it produced, was not calculated to prove its invariable success.

Since the time of Mr Baynton, many different plans have been proposed for effecting the permanent cicatrization of ulcers on the legs; various improvements have been effected; and various

means have been adopted to supply defects and to rectify errors in previous methods of treatment. Of the different individuals by whom these methods have been advocated, Mr Walker gives a short view. Among the most important of them, Mr Higginbottom's method, by the employment of nitrate of silver so as to produce an adherent eschar, has been found very beneficial. Various important improvements were also introduced by Mr Spender, and of these we gave a detailed account several years ago. The most recent propositions, previous to those of the authors whose treatises are under consideration, are those by Mr Stafford, Mr Skey, Mr Eccles, Mr Edward Johnstone, Mr John Davis of Hartford, Mr Maxfield, Mons. Guyot, Mons. Comté, Mons. Gerdy, Mons. Roche, and Sanson. It is unnecessary to advert to all the plans of these several authors. Our chief business at present is with the new propositions brought forward by Mr Walker, Mr Chapman, and Mr Critchett.

The plan proposed by Mr Walker appears to be a very simple, and, at the same time, a very effectual one. It consists in applying to the surface of the sore, by means of a suitable apparatus, the vapour or fumes of sulphur and iodine combined.

The apparatus itself is extremely simple. It is composed of a series of mahogany boxes, having in the cover of each a round aperture for the reception of the limb. Each box communicates below with the engine-room of Mr Walker's establishment; and, from this apartment, heated air or warm vapour can be passed into the apparatus as the case may require. On the bottom of the box is placed a plate of heated metal, and on the latter the powder of iodine and sulphur, which is to be volatilized by heat. The relative quantities of the medical agents; the time during which the remedy is to be applied; the temperature at which it is applied; the necessity, at the same time, of employing heated dry air or vapour; these and many other circumstances are regulated by the peculiar indications of each individual case, and cannot be embraced in any general description. For ordinary purposes, a powder, containing from two to three grains of iodine, and half a drachm of the flowers of sulphur, will be sufficient; but the relative proportions of the remedies must be varied, according to the expediency of giving preponderance to the action either of the iodine or the sulphur. This can be learned only by a careful examination of the case, and by experience. Sometimes it is found that a small proportion of iodine exerts a strongly-stimulant influence on the sore; in other cases the sulphur seems to act most beneficially.

The time during which the fumigation is to be continued can be determined only by experience and experiment. As a general rule, fifteen minutes may be stated as the average time, but many sores



and many patients will not bear this moderate application ; while, on the other hand, either from the constitution of the individual, or the indolent and insensible condition of the ulcer, it is requisite in other cases to prolong the period of fumigation, to repeat it frequently, and to increase the proportion of the materials employed.

The degree of temperature Mr Walker represents to be not without influence. A moderate temperature produces a soothing effect ; while a high temperature unquestionably excites ; hence the temperature of the box, should be regulated by the state of the ulcer, as to sensibility and the feelings of the patient. An indolent sore requires a considerable degree of heat, while an inflamed or irritable ulcer should be treated by the lowest temperature at which sulphurous gas can be generated.

The number of fumigations necessary for the cure of any given case of ulcer, cannot be determined beforehand. In some cases fumigation every second day is all that the patient can bear with advantage ; in other cases the fumigation must be repeated every day, and in some cases twice daily. The chief rule for regulating this part of the treatment is the effect which the remedy produces on the disease. So long as the plastic exudation continues to be formed on the surface of the sore ; so long as its edges are surrounded by the white border ; and so long as the contraction of the ulcer with cicatrization proceeds ; so long we may be confident that the case is advancing favourably towards a cure.

The application of remedies by fumigation in the manner now mentioned, in order to effect a favourable change in the surface and actions of habitual sores, is not new. Mr Walker allows that it was recommended by Mr John Banester more than two hundred years ago. It seems, indeed, to have been rather a common remedy at that time, for this mode of medication is recommended not only by Banester, but by other authors. To us it appears to be extremely well calculated to attain the object in view, and to form a very efficient remedy in cases of old indolent or otherwise obstinate ulcers, over which ordinary remedies have little or no influence.

Mr Chapman does not confine his treatment of ulcers of the lower extremity to any one method or mode of application. After a general view of the sources of intractability, that is, the impediments to treatment in ulcers, the author distinguishes intractable ulcers into two orders : 1<sup>st</sup>, ulcers which are intractable from constitutional causes ; and, 2<sup>dly</sup>, ulcers intractable from local causes. He next considers the various remedies and modes of treatment which are employed for accomplishing the cicatrization of ulcers. He then takes a short view of the effect of cold and warm water

dressing. Cold water dressing he seems to regard as most generally applicable. For indolent and callous ulcers he proposes the method by Dr Bresciani de Borso, who cicatrizes an old ulcer by establishing near it a new one, by means of caustic potash. Of the employment of electricity in the method adopted by Dr Golding Bird and Mr Bransby Cooper, he speaks in favourable terms. The tepid water dressing, aided by the occasional application of red precipitate, he considers as suitable to the irritable ulcer. For varix and varicose ulcers, the usual remedies are recommended.

By cold water dressing Mr Chapman understands charpie, or rather minute portions of sponge dipped in water, ranging between 40° and 70° of the thermometer of Fahrenheit. By tepid water dressing he understands the employment of water at the temperature of from 80° to 95° of Fahrenheit.

The method of applying the cold water dressing is described in the following passage:—

“Instead of lint, therefore, in such circumstances, I make use of soft sponge, torn up into very small shreds, and soaked in water; these are dropped lightly into the ulcer, and covered with a single layer of lint, over which the bandage is carried, as in shallow sores; gentle support being thus conveyed to the entire surface, and tone communicated to the minute vessels, granulations spring up uniformly and vigorously, and fill the hollow of the ulcer, often with surprising rapidity, of which Case XVII. is an example. The sponge acts, as far as its compressing power is concerned, on the same principle as the wax dressing poured into deep ulcers, in the manner suggested by Mr Stafford; according to my experience, however, it is not only a more convenient mode of effecting the same object, but accomplishes it more speedily and completely. The shreds of sponge should be well soaked, and lightly distributed, in order to avoid any ill consequences from too much pressure by their subsequent expansion.

“Under this simple plan of treatment, I am satisfied that the granulation and cicatrization, in a large majority of cases of indolent ulcers of the leg of long standing, even when attended with a high degree of irritability, will proceed more favourably and expeditiously, and occasion less inconvenience to the patient, than under any other method whatever.”—Pp. 84, 85.

Mr Chapman, nevertheless, trusts not solely to this method. He avails himself of the aid of the supporting plaster of Mr Baynton, which he commends highly. Nitrate of silver is also employed to repress exuberant granulations.

The method of employing the tepid water dressing is described in the following passages:—

“The leg is then to be bathed for some time with tepid water,

decoction of poppies, or a fomentation composed of equal parts of poppy decoction and spirit of wine, and the ulcer dressed with a piece of soft lint dipped in the same. A watery solution of opium may be used when the pain is very severe. Moistened strips of soft linen must next be folded round the leg as smoothly as possible, in the same manner as in the treatment by cold water dressing, and drawn tightly enough to prevent plaits forming under the bandage. Over them the roller, soaked in warm water, is to be carried lightly, and the whole freely bathed for some time with tepid water, or poppy decoction. The tepid bathing must be persevered with until the pain, which is generally severe on the first application of the bandage, is relieved, and repeated as often as any exacerbation of the morbid sensibility occurs, the leg being enclosed, during the intervals of the tepid bathing, in a sheet of oiled silk. During the first twenty-four hours, sometimes for a longer period, it is expedient to confine the limb to the horizontal position; before that time has elapsed, however, the morbid sensibility will have in a great measure disappeared, and the patient will begin to experience the beneficial operation of the gentle support upon the part. Permission may now be given to use the limb cautiously, still continuing the tepid affusion. After a time, to be determined by the state of the sore, and the feelings of the patient, cold affusion may be tried, and, if not found to disagree, may be advantageously substituted for the tepid fomentation; not unfrequently, however, this change provokes a return of morbid sensibility, and tepid dressing must be pursued until the cure is completed.

“The necessity for the oiled silk envelope is still greater with tepid affusion than when cold water is employed,—evaporation will otherwise take place so rapidly, that a greater reduction of temperature will be ultimately effected than by the direct application of cold, none of that tendency to reaction, which counteracts the depressing influence of cold, being excited by tepid water.”—Pp. 101–103.

Mr Chapman's work contains an appendix of thirty-one cases, illustrating the characters of the different forms of ulcer, and the adaptation to them of different methods of treatment. The applications appear to be judiciously applied, and it is a great recommendation that the patients were not confined or prevented from using the limb during treatment. These cases form the best comment upon the general principles of Mr Chapman's work.

The Lectures by Mr Critchett contain a systematic view of the causes, symptoms, and treatment of ulcers of the lower extremity.

The method of treatment preferably recommended by Mr Critchett is founded on that originally inculcated by Mr Baynton, but modified by improvements chiefly in the mode of applying bandages, introduced and practised by the late Mr John Scott.

It is well known that Mr Scott excelled much in the peculiar mode in which he applied plaster and bandage, in the instance of diseased joints; and that to this excellence he owed much of that success which attended his treatment of these disorders. Mr Crichton thinks, that though Mr Baynton's method contains the general principle, yet it does not carry out the method in all its details, and he conceives that this was only effected by Mr Scott of Bromley and his son Mr John Scott.

After some general remarks on the method of Mr Scott, and showing how imperfectly its peculiar details seem to be known, and its merits are recognised by the profession, Mr Crichton proceeds in the following manner:—

“If, then, we would justly apportion merit, we must limit to Mr Baynton the credit of being the first to recommend the use of adhesive strips in the treatment of ulcers. Here, I contend, his claim ceases. To the late Mr Scott of Bromley is due the higher honour of working out the true principle upon which alone these adhesive strips can be safely and successfully used; to the late Mr John Scott the profession owes the publication of these principles, and the teaching of these views to his pupils, many of whom have for years vindicated the advantages of this method of treatment, by the success which has attended their labours in this department of surgery; and to the London hospital belongs the merit of having afforded abundant materials, whereby the correctness of the principle and the efficacy of the practice have been extensively worked out and tested, and pupils practically qualified to carry out this system. Whence, then, I would ask, can the reassertion of this principle, so strangely neglected or unobserved by all professional authorities, more legitimately emanate, than from that hospital where it was first publicly taught and practically substantiated?

“I will now proceed to explain to you, in detail, the method I recommend you to adopt, in order to accomplish a complete support of the entire limb. You must seat your patient opposite to you, and support his foot upon a small stool, about a foot and a half in height, and so constructed as to receive the point of the heel, and leave the rest of the foot free. You should be provided with strips of plaster, about two inches in width, and varying in length from twelve to eighteen inches, according to the size of the limb. The best material for this purpose is the simple emp. plumbi of the Pharmacopœia, spread upon soft, unglazed calico, and free from resin, which is often introduced to increase its adhesiveness, but which is very liable to irritate the skin. If the plaster be well made, and of the best materials, it will adhere perfectly; I have often found it unmoved for many weeks, and even months. It is convenient to provide yourself with a metallic warmer, made with a flat top, upon which you can lay three or four pieces, heated either by hot water or by small lamps, which are better, if you require it for any length of time. This form of warmer is far preferable to the circular one ordinarily in use, saving both time and trouble. But to proceed. You then

take the centre of the first piece, and apply it low down to the back of the heel, and then, with the flat of both hands, press the plaster along both sides of the foot. This plan is very preferable to taking hold of the ends, and endeavouring to apply them, as it ensures a perfectly smooth adaptation of the plaster to the part, and also because it enables you to regulate that very important point, the amount of tightness you may wish to employ. As you proceed with the remainder, you must always remember the principle is to make one portion hold on another; you must therefore alternate them round the foot and the ankle. Your second piece should be placed in a similar manner underneath the heel, and then carried upwards, at a right angle to the last, so as to cover a portion of each malleolus. The third piece should be again applied to the back of the heel, overlapping the first by about one-third. The fourth piece under the foot, and carried upwards, each piece being pushed along, so as to allow it to take its own course; this must be continued until the foot and ankle are covered; the strips must then be carried in a similar manner up the leg, increasing in length as the calf increases, and extending as far as the knee, and in some few cases even above this. A calico bandage, about three inches in width and eight yards in length, varying, however, according to the size of the limb, must now be applied in the following manner:—if it is the right leg, the bandage should be held in the right hand in commencing, and *vice versa*. This is in order to bring the folds, which are necessary as you ascend the leg, on the flat part of the tibia. The first turn should be made round the ankle; the second round the foot, near the toes; the third round the lower part of the ankle, near to the *os calcis*; the next round the foot, overlapping the second about one half; the fourth round the ankle, so as to cover the loose upper end of the fold that passes round the back part of calcis; the fifth round the foot. And now you begin to ascend the leg in spiral folds, each overlapping the last rather more than one half. As the limb enlarges, it is necessary to give a turn to each fold of the bandage; this is done by placing the thumb of one hand on the spot where you desire to limit the extent of the fold, and give a peculiar sweep of the other in which the bandage is held, so as to leave the bandage perfectly smooth. This must be repeated until the limb no longer increases in size, when two or three folds may be carried obliquely downwards, so as to fix the remainder; the end must be secured with pins, and the limb is banded, and is supported in the most complete and efficient manner that human ingenuity has yet devised. None but those who have practically tested the matter can estimate the immense difference between mechanical support so obtained, and that which the most accurately-applied bandage, when used alone, can accomplish; it is, in fact, far greater than mere reasoning upon the subject would lead you to expect; and whilst it accomplishes all that rest can do for the ulcer, in many cases it does a great deal more, enabling the patient to pursue his ordinary avocations, and at the same time healing the wound more rapidly, and far more lastingly, than the most complete rest would effect. In carrying out this method, we must remember that we have two objects to ac

complain: the one is, to obtain a healthy circulation through the entire limb, and the other, to act upon the dilated capillaries immediately surrounding the wound. Both these objects are frequently accomplished by the general support I have just described; but it sometimes happens, particularly in small ulcers situated in the hollow between the *malleolus* and the *os calcis*, that the diseased vessels immediately around the wound require an amount of pressure which the rest of the limb would not bear. Under these circumstances very great advantage is derived from applying, previous to the support I have just described, some pieces of strapping, about six inches in length and two inches in width, in a crucial manner, over the wound, so as to extend a few inches above and below it; these pieces, as they do not encircle the limb, may be applied with all the force and tightness the surgeon can exert. If this plan be super-added to the other in certain cases, it is of great assistance to the surgeon, enabling him to combine considerable local pressure upon the weakened and distended vessels immediately surrounding the sore, with gentle mechanical support to the entire limb. But it may be asked, is this complete support of the entire limb always necessary? I answer, certainly not. When the limb is in an otherwise healthy state, the congestion confined to the circumference of the ulcer, and the ulcer is situated tolerably high up in the limb, short strips applied so as to cover a few inches above and below the wound, will answer every purpose; but it is of the utmost importance that these pieces should not surround the entire limb, for reasons that I have before insisted upon; and I would lay it down as a rule without exception, that in every case in which it is necessary to apply strapping entirely round a limb, it must never be partially applied, but must encompass and support every part of the leg."—Pp. 24–31.

In short, the grand secret of successful treatment in the management of ulcers of the lower extremity, according to Mr Critchett, consists in the application of plaster and bandage as tight as the patient can bear, and so uniformly as to encompass the whole limb in one equable and supporting enclosure. The immediate effect of this enclosing application is, he says, to remove all pain, and to enable the patient to move about with ease; and in this movement he is to be encouraged, as it hastens the cure. Various cases he describes, illustrating the beneficial effects of this mode of treatment in every form and variety of ulcer on the lower extremity. Even in the very troublesome and perplexing varicose ulcer, or that with general disease of the veins, he represents the uniform tight enclosure to be the only safe and efficient method:—

"You must continue to apply the strapping with the same tightness at the close of the case as you did at the commencement. During the progress of your treatment, you often receive a curious and unmistakable evidence that extreme pressure is here required. A patient will tell you, that the only time that he feels pain in the wound is early in the morning, when the binding is loose, but that

as soon as he has moved about a little, and the leg has increased so as to distend the encasement you have made for it, the sore is perfectly easy. It is a good rule, under such circumstances, to direct your patient to rest the leg, on the morning it is to be dressed, until he comes to you, that you may in this way obtain a still further advantage over the dilated and distended veins. Still we must regard a case of this kind, even when perfectly healed, as somewhat analogous to a rupture, inasmuch as it always requires some palliative treatment to prevent a return of the sore. It is a safe plan to continue the strapping for two or three months after the ulcer is healed, and I have met with some few cases in which it was found desirable to continue this method constantly; for it must ever be borne in mind, that for accuracy of adaptation, and complete, unyielding support to every part of the limb, there is no contrivance that human ingenuity has yet suggested, at all to be compared to skilfully-applied strapping. In less severe cases, a slighter and more imperfect form of support answers the purpose. The elastic-stocking bandage is easily applied, but is apt to give way in parts where you require most pressure. What I much prefer is a well-fitting elastic stocking, where the patient can afford the money to purchase it, and the time to lace it, as it must be removed at night, and reapplied before quitting the bed in the morning."—Pp. 83, 84.

The menstrual or uterine ulcer suggests some useful observations, and a case is given showing what caution is requisite in managing the general and constitutional symptoms in persons, in whom an ulcer on the leg has this connection.

Mr Critchett speaks of the water-dressing not in the confident terms in which Mr Chapman eulogises that method of treatment. He allows that it is at once cleanly, agreeable, and efficacious. But he does not ascribe to it the universally and exclusively beneficial results in which many persons believe.

In conclusion, the three performances now introduced to the reader deserve attention from the correct and rational views which they present; and it cannot be denied that they promise to be useful to the surgical practitioner, by explaining and illustrating various practical details, to which too little attention is often paid. The surgeon should, above all, study carefully the mode of applying plaster and bandage according to the method observed by the late Mr John Scott.

ART. V.—*Surgical Anatomy*. By JOSEPH MACLISE, Surgeon.  
Fasciculus I. II. III. London, 1849. Twelve Lithograph  
Coloured Engravings.

THERE is perhaps some ground for the complaint which the author of this excellent performance makes of the great difficulty

of presenting any novelty in the department of surgical anatomy after the researches of the laborious men by whom that science has been so skilfully cultivated. Much, nevertheless, may be expected both to be learned and to be communicated, by going over the trodden ground and collecting and viewing in different lights the great leading features of each division. No one promises better to impress upon the mind of the surgeon old views which may be partially known, and new views which ought to be fully known, than Mr Maclise; and the work, of which the three numbers are now before the public, shows that, under his guidance, the operating surgeon may most profitably spend his time and study.

Mr Maclise informs us that his object was to indicate the interior through the superficies, and thereby to illustrate the whole living body which concerns surgery through its dissected dead counterfeit. The purpose of the teacher of surgical anatomy is, indeed, to render the human body, as if it were transparent, and to enable the surgeon to paint to his mind, the relative situation of all the organs, muscles, blood-vessels, and nerves which lie beneath the surface. He who can best and most accurately do this is the most able surgical anatomist; and if he do not form the most dexterous operator, he possesses at least the essential knowledge and qualifications to become so.

In the three fasciculi now before us are contained twelve lithographed engravings. Of these, the first nine illustrate the relative anatomy of the thoracic cavity of the superficial cervical, and facial regions, and the position of their principal blood-vessels and nerves; the subclavian and carotid regions; the sterno-clavicular or tracheal region; the axillary and brachial regions; the differences of the axillary region in the male and female; the arteries, veins, and nerves, at the elbow and fore-arm; and the surgical anatomy of the wrist and hand.

In the tenth plate is given a very correct and beautiful view of the relative position of the cavities of the cranium; the nasal fossæ; the mouth, and the pharynx; part of the larynx; and the relation of the spinal to the cranial cavity.

The eleventh plate illustrates the relative position and superficial view of the organs of the chest and abdomen; and the twelfth plate presents a view of the deep-seated organs of the same cavities.

The whole of these plates are executed with great fidelity and accuracy, and reflect the highest credit upon the anatomical knowledge of Mr Maclise, as well as upon his talents as a draughtsman. It is manifest, that he has very frequently dissected the parts concerned, and that he has studied the best and most



effective positions to place them before the eye of the beholder in a correct and impressive light.

To each plate is attached a descriptive commentary, explaining all the most important anatomical facts, and showing their surgical applications. These commentaries we recommend strongly to the perusal of the student, both of surgery and medicine. The physician, indeed, will find in these commentaries many instructive observations to guide him in the examination of the viscera of the chest and abdomen.

These plates, we may add, will form a valuable acquisition to practitioners settled in the country, whether engaged in surgical, medical, or general practice.

ART. VI.—*Pathology of the Human Eye.* By JOHN DALRYMPLE, F. R. C. S. London, 1849. Fasciculus I, II., with Coloured Plates.

IN these engravings, Mr Dalrymple undertakes to illustrate by coloured drawings the principal diseases of the eye and its appendages. He does not profess, he states, to give a systematic treatise on the subject, but to illustrate in a series of drawings, the various forms of ophthalmic disease as they occur in nature with such explanations as may be adequate to identify them with the symptoms, and with the general treatment of the case.

Mr Dalrymple has for many years enjoyed great advantages in observing and treating various forms of ophthalmic disease at the Royal Ophthalmic Hospital in Moorfields; and it appears that the late Mr John Scott, who had made an extensive collection of drawings of these diseases, bequeathed to the present author this collection, that it might not be lost to the profession. For this duty Mr Dalrymple is evidently extremely well qualified.

Each of the two numbers before us contains four plates, illustrating mostly the inflammatory affections of the eyelids and conjunctiva; some of the eruptive diseases of the eyelids; and various affections of the lachrymal apparatus. The affections here illustrated are not connected by any natural alliance; but, as specimens of the diseases which they represent, the engravings will be found most faithful and instructive illustrations.

The sixth plate is devoted chiefly to diseases of the lachrymal apparatus; and exhibits correct views of abscess over the lachrymal sac; abscess of the sac itself; fistula lachrymalis; and one or two eruptive diseases.

The eighth plate is a good example of scrofulous ophthalmia. Upon the whole, these drawings possess qualifications which recommend them strongly to the attention of the surgeon.

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## PART III.

### MEDICAL INTELLIGENCE.

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#### I. MEDICAL THERAPEUTICS.

*Discharge of a Gall-stone of unusual size through the Abdominal Parietes.* By Dr KLEMM of Greunen. Berliner Med. Central Zeitung, 1842. No. 25. (Archives Generales 1843, T. ii. 4ieme Serie).—In March 1841, Dr Klemm was called to a woman of 70 years, robust and well formed, who complained for some time of cough, and of a tumour in the right side of the epigastric region. Attentive examination enabled the physician to recognize, towards the right hypochondriac region, below the false ribs, a hard tumour of the size of a goose's egg, movable, yet adherent to the external integuments of the abdomen. Pressure caused pain, which extended into the hepatic region. This tumour, according to the account of the patient, had been there about two months, at the same time as the cough, of which she complained. The health was good. There was no fever, nor any symptom of disease of the liver. All the secretions and excretions proceeded as in the normal state. The appetite was good; the skin and eyes presented no perceptible coloration. Emollient and soothing remedies were prescribed.

At the end of fourteen days the tumour swelled, became red and painful, and evinced a disposition to suppurate. Three days after, fluctuation was distinct; and an incision procured the discharge of about two ounces of sero-purulent fluid, yellowish, and mixed with blood. The tumour, nevertheless, retained its shape and firmness. On examination by probe, the operation recognized, at the depth of about one inch and a half, a hard body, which could be circumscribed by sinking the probe to the depth of nearly three inches.

After having suitably enlarged the incision, M. Klemm was able to extract this foreign body, which was enclosed within a very dense cyst, situate between the muscles of the abdominal parietes, and which was found to be a biliary concretion of considerable size. The gall-stone had the shape of the gall-bladder, though it was larger, being about the size of a hen's egg. Its external surface was of a deep brown colour; internally it was of a bright yellow, and presented a radiated structure. In the centre was a small nucleus, from which proceeded transparent, almost crystalline radiations. The matter of this concretion was friable, very rapidly combustible, and burned, diffusing a peculiar odour. It was completely dissolved in oil of turpentine and sulphuric ether. It weighed 6 gros and 2 grains, = 8 drachms 2 grains; and its specific gravity was below that of water.

After the extraction of the concretion and a suppuration somewhat lengthened, the wound was completely healed; and since that time the patient has enjoyed excellent health.

M. Klemm, the reporter observes, was unable to follow the different stages through which the tumour in this case passed. It is probable, notwithstanding the absence of details, there must previously have been inflammation of the gall-bladder, followed by its adhesion to the abdominal parietes. Ulceration having followed, the concretion might have remained for some time without indicating its presence by inflammatory symptoms; and at a later period the phenomena attending diminution may have ensued. The only remarkable circumstance is the complete absence of the symptoms of this transition. As to the size of the concretion, it is probable that the gall-bladder must have augmented its size to an unusual degree, in order to hold the concretion.

## II. SURGICAL THERAPEUTICS.

*On the Separate and Combined Action of Cold Douches, and Movements gradually forced in the Treatment of incomplete Ankylosis.* By M. Le Dr L. FLEURY, Assistant to the Faculty of Medicine, Paris. (Acad. des Sciences, 10 Juillet 1848, et Archives Generales, 4 serie, T. xvii. 1848.)—"The name of *Ankylosis*," says Boyer, "is given to that condition of a diarthrodial articulation, in which the movements of the bones composing the joint are entirely abolished or very much confined, whether the member be in a state of flexion or of extension. Ankylosis is distinguished into true or complete, and false or incomplete. In true ankylosis, the bones are so united and agglutinated together, that they form one single piece, so that the motions of the joints are absolutely abolished for ever. In false ankylosis, the bones are not agglutinated; they have a certain degree of mobility; and the movements of the articulation may be re-established, when the cause by which they were prevented no longer exists."\*

This mode of defining, distinguishing, and considering ankylosis is controverted by M. P. Boyer. This surgeon defines ankylosis to be "the fusion or the agglutination of the bones forming a moveable joint, causing complete and permanent impossibility of the movements of the articulation." The disease described under the name of false *ankylosis* is not ankylosis, he asserts; the surgeon is thus made to place together lesions very dissimilar,—lesions which it is important not to confound, and which ought to be studied separately as affections which simulate ankylosis. Under this title M. Boyer enumerates inflammation of the articulations, white swellings, luxations, fractures, muscular contractions, firm cartilaginous cicatrices, and tumours of different characters situate near the articulations.

This new manner of considering ankylosis appears to M. Fleury to be too absolute, and therefore dangerous in a clinical point of view. It is further not strictly correct in an anatomico-pathological sense, on which it professes to be founded.

Can the surgeon, he asks, in all cases be sure of the nature and seat of the changes which impede the free movements of a joint? The complete absence of movements does not prove the existence of true ankylosis. It is uniformly possible to distinguish the simple adhesion of articular surfaces from the agglutinations or fusion. The lesions accompanying ankylosis are often complex; they are at the same time extra-articular and intra-articular; they include the muscles, the ligaments, the synovial membranes, and often the bones.

Ankylosis, viewed in itself, is, Sanson observes, not so properly a disease as the effect of a series of other affections; and it may succeed all those which destroy any one of the conditions, without which an articulation cannot be moved.

\* *Traité des Maladies Chirurgicales, et des Operations qui leur conviennent*; par M. le Baron Boyer. Tome Quatrieme. Seconde Edition, A Paris, 1818. Chapitre xxi. p. 553.

This definition M. Fleury thinks quite correct. It shows that the term ankylosis belongs to surgical semeiology; and ought there to retain its place on the same ground on which the terms delirium, vomiting, &c. belong to medical nosology. We must not, however, thence conclude, that, in the case of ankylosis, it is unnecessary to investigate the cause, the particular lesion which impedes the free motions of the articulation. Further, if the exact cause of ankylosis eludes observation, or if, after recognising and overcoming it, the constraint of the movements continues, he ought, in that case, to view the ankylosis by itself, and attempt special treatment of it; in short, to treat it as a symptom.

Numerous facts show that, in thus proceeding, it is possible to cure completely cases of ankylosis, regarded as previously incurable by the most eminent surgeons. The diagnosis has been weakened by the treatment; as this has proved that, instead of being caused by agglutination and fusion of the articular surfaces, the abolition of the motions was caused only by simple adhesion of these surfaces, an alteration in the synovial membranes, in the capsules and ligaments, in the muscles or bones.

He then considers the curative means for removing incomplete ankylosis considered in itself.

Most surgeons allow that, for curing incomplete ankylosis, art is often impotent, and this in the direct ratio of the duration of its existence. "It is impossible to expect any satisfactory result," says M. Bonnet, "unless when the articular surfaces retain nearly their normal figure, when they are covered by a layer of fibrous tissue of new formation, smooth, and allowing an easy gliding motion, and when the adhesions within and without the articulations depend only on cellular or fibrous tissue, possessing a certain degree of extensibility. It must nevertheless be observed, that, even in these favourable cases, imperfect results only are obtained."

Emollient applications, douches with thermal waters, vapour baths, mentioned by all authors, are adjuvants of limited efficiency.

Artificial graduated movements, effected either by the hands of the surgeon or by the aid of machines or apparatus, constitute a therapeutic method which prevents some chances of success; and M. Malgaigne is known to have obtained prosperous results from this means. It must be admitted, however, that this treatment is often impracticable or inadequate.

Its impracticability is felt on various occasions. Several patients cannot endure the forced movements, in consequence of the severe pains which they cause, whatever be, in other respects, the care, prudence, and moderation with which the surgeon conducts them. In other instances, movements the most limited induce intense inflammatory symptoms, which returning on each attempt, end in becoming an insurmountable obstacle to their employment. These results the author observed in the case of a young girl, for whom he had requested the assistance of M. Malgaigne; and they were also induced in the case of two patients whose history is given in the essay (3 and 4).

The inadequacy of the method by artificial graduated movements is not less certainly proved. These forced movements operate principally as mechanical means of extension. Their action on the lesions which have taken place in the synovial membranes, the capsules, the ligaments, and muscles, is only indirect and inconsiderable. They modify neither with sufficient promptitude nor energy, the capillary circulation, nutrition, the absorption of composition, and decomposition. They do not completely recal the synovia to the joint,—pliancy and elasticity to the fibrous tissues. They do not sufficiently arouse the enfeebled or extinct contractility of the muscles. When forced movements are exclusively employed, the duration of the treatment is always very long, and often, as observed by M. Bonnet, only imperfect results are obtained.

Several patients affected with ankylosis having presented themselves at

the hydro-therapeutic establishment of Bellevue, M. Fleury thought that cold douches might be serviceable; and the results obtained by the separate action of these, or by that combined with forced movements, are given in the present memoir.

M. Bonnet has made no use of these cold douches in the treatment of ankylosis; and the method is not mentioned in the works of Scoutetten, Schedel, Engel, Baldon, Lubansky, &c.

M. Fleury fulfils, he conceives, two principal and different indications by means of these douches.

1. By employing cold water as an exciting agent of the capillary circulation, he proposed to re-establish the secretion of synovia, to act on organic absorption and nutrition, so as to restore to the fibrous tissues their pliancy and elasticity, to the atrophied and more or less paralyzed muscles their bulk and their contractility, and in short to restore to their normal conditions the soft and osseous parts.

2. By employing cold water as a sedative agent, M. Fleury was desirous to render practicable or less painful the necessary artificial movements, and to reduce, to the smallest possible rate, the irritation more or less intense which they uniformly induce.

Four cases are given, of which extracts are subjoined.

1. M. I., residing at Paris, Rue d'Antin, aged 58 years, of tall stature and athletic figure, always enjoyed good health, and was never affected by rheumatism. At Naples, during 1845, he was obliged to rise during the night to attend a sick child, and next morning he felt in the right shoulder-joint acute pain, which was aggravated by the slightest movement of the limb. This pain continued several days. No treatment was adopted except that of leaving the right arm in a state of rest.

From this time to the month of July 1847, M. I. felt only intermittent pains, not very intense so long as the movements did not exceed certain limits; but as soon as they were exceeded, acute pains were felt. The freedom of the movements was progressively abridged, to the degree of rendering impossible certain habitual acts of daily life; and in this state the patient applied for assistance.

At this time, the parts were in the following state:—The articulation was not deformed; the deltoid had not sensibly diminished in size; limited movements were free, passed into the articulation, and were attended by no pain. But beyond limits now specified, they could not be performed. When, the fore-arm being inflected, the patient wished to carry the arm upwards and outwards, the elbow could not be raised to the level of the shoulder, so that the patient, who is a great sportsman, was much constrained in the act of shouldering his fowling-piece; the hand carried backwards and upwards scarcely reaches the inferior angle of the scapula; when the two superior extremities are carried directly upwards, the hands being approximated to each other, the difference in level between them, to the disadvantage of the right, amounts to 10 centimetres. The patient cannot help himself suspended by the right arm; sudden and violent movements are entirely prohibited; thus he cannot, with the right hand, throw a stone, or correct his pointer. Forced movements, spontaneous or artificial, excited acute pain.

On the 6th of July 1847, M. I. began treatment, consisting in receiving, twice daily, for the space of about five minutes, a general shower-bath, and a local energetic douche of 3 centimetres in diameter, directed on the articulation of the right shoulder.

On the 15th, some artificial movements had been executed, though with difficulty, in consequence of the muscular power of the patient, and the resistance he gives to manœuvres causing him acute pain.

On the 30th, the movements were performed with more ease and less

**pain.** The patient endeavours daily to increase their extent by spontaneous efforts, which he is no longer afraid to make.

On the 15th of August, there was only a slight difference between the movements of the right upper extremity and those of the left. The patient could, without uneasiness, perform the act of throwing a stone; and this movement he repeated several times, to give it more force and extension.

On the 6th of September the cure was complete. The movements of the right limb were as extensive as those of the left. Pain was gone.

On the 5th May 1848 the cure continued perfect. M. I. pursued his sporting amusements during the whole autumn, without feeling the slightest constraint or the smallest pain in the right arm,—the movements of which possess all desirable extent and facility.

This case the author regards as an example of ankylosis in the first degree. The articular surfaces were healthy, and free from adhesion. The slight rheumatic inflammation which was the origin of the disorder, acted probably only on the muscles, and, at most, diminished the extensibility of the fibrous textures. These lesions, nevertheless, were sufficient to diminish remarkably the movements of the arm, to render every effort very painful; and the disease had already continued for two years.

The cold shower-bath and douches were, in this case, almost the exclusive instrument of cure; and under their influence the movements were restored, pain disappeared, and in two months a cure was completed.

2. The next case shows, he conceives, the influence exercised by the douches, on organic absorption, and through that channel, or certain osseous and extra-articular alterations, of which ankylosis may be the result.

A walk-maker, aged 56, of small stature and feeble constitution, was subject from the age of 17 to rheumatic pains. Eighteen months previously, pain and swelling attacked suddenly the right foot, and continued for nearly six months, notwithstanding the use of poultices, applications of tincture of camphor, and douches of aromatic vapours. Shortly after the disappearance of these symptoms, that is, about twelve months, pain and swelling attacked the right knee, and were opposed at first by bleeding by means of leeches, afterwards by application of several blisters. These remedies procured relief; but the knee continued swelled, and occasionally painful; the movements became difficult, progressively less extensive; and the practitioner advised the patient to repair to Paris, to be admitted to an hospital. Resting at Meudon for a few days, he was seen by M. Le Dr Baud, who recommended him to M. Fleury, on the 14th May 1847.

The right knee was then remarkably deformed. It was large; measured at the level of the middle of the knee-pan, it was 6 centimetres in circumference larger than the left knee. No effusion had taken place within the articulation. The skin was red and hot; the soft parts swelled, hard, resisting; but it was observed that the change in shape was caused chiefly by enlargement of the femoral condyles, the texture of which was hypertrophied. The appearance of the joint was that of a white swelling in the first period of its growth. The leg and thigh were atrophied, and presented an unpleasant contrast with the bulk of the knee. The patient stated that for six months the disease had remained stationary, and that the appearance of the limb had undergone no change.

The leg was bent so as to form with the thigh an angle of about 140 degrees. The patient could move it only in a very slight degree. If, by fixing the thigh, it was attempted to raise the leg, the latter might be a little extended; but flexion was altogether impracticable; and it was observed that the impediment to this motion was seated chiefly in the enlargement of the femoral condyles.

In consequence of this state of the knee-joint, the right leg was shorter than the left, and well-marked lameness was the result. The patient could

walk only by the support of a staff, and, nevertheless, he took more than two hours in coming from Meudon to Bellevue.

The patient raised the limb with much difficulty; it felt, he said, like lead. He felt pains almost continual, and which were aggravated during the night, and when the foot touched the ground.

Treatment was commenced on the 16th of May 1847. Twice daily, the movable double 3 centimetres in diameter, was directed for ten minutes at a time on the right knee. The first douches had been a little painful, and caused redness and swelling; but under the use of cold compresses frequently renewed, these symptoms disappeared. On the 25th May, the douche was readily endured; pains were less intense and less continued; the patient supported himself less on the staff. He could perform, without pain, some small movements of extension and flexion.

On the 22d June, the pains had almost wholly disappeared; the volume of the knee had diminished by 25 millimetres; the skin was less red, more movable; the subjacent parts were less hard. The patient was able to extend the leg completely; the movements of flexion were more easy and more extensive. Walking was much improved, and was more rapid. The patient came from Meudon to Bellevue in less than one hour. Lameness was diminished, and he could walk a few steps without the aid of the staff.

On the 16th of June, during fifteen days, improvement had advanced less rapidly. The soft parts had entirely returned to their normal state, but the femoral condyles were still voluminous, and prevented flexion of the leg. In place of the movable douche was substituted a vertical douche greatly more powerful, and 5 centimetres in diameter. By the 26th June, the effect of the vertical douche had been prompt and successful. The volume of the knee had diminished between 27 and 28 millimetres; the flexion movements were greatly more extensive; lameness was almost gone. The patient walked to Bellevue with his staff under his arm; pain was gone.

On the 10th of July, between the volume of the right knee and that of the left there was a difference of 3 millimetres only. Flexion of the leg was nearly perfect. The patient was able to walk firmly and to run,—in short, to perform energetic and extensive movements. He had broke his staff, and in twenty minutes walked from Meudon to Bellevue. Lameness and pain were gone. The patient was decided on returning homeward, and took leave, resolved to travel forty leagues on foot.

Forced movements, the author thinks, were in this case altogether useless, and they were not employed. They could nothing avail against the osseous hypertrophy which impeded the movements; and there was neither adhesion nor retraction. The movable douche caused the swelling of the soft parts to disappear rapidly; and though it was unavailing against the change in the bone, that speedily yielded under the use of the vertical douche, which induced a very active process of interstitial absorption.

In the two cases now recorded, cold douches were employed alone, and they were sufficient to effect a cure, by giving activity to the capillary circulation and modifying absorption, and the vital properties of the tissues. In the two next cases, the use of douches were continued, with that of forced movements.

3. Madame V., resident at Paris, 58 years of age, of slender constitution, with marked nervous temperament; general health shaken by very assiduous labour, sedentary life, and grief for the loss of two children who had died within eighteen months. Madame V. never had rheumatism; she suffered from habitual and obstinate constipation; digestion was laborious; every winter she had several attacks of bronchitis, which caused much distress.

Ten years previously a considerable abscess formed on the left axilla.

Multiplied and extensive incisions were made by M. Larrey. These were followed by scars in the form of bridles, which diminished considerably the extent of the movements of the left arm.

In the month of June 1845, Madame V. was descending from her carriage, supporting herself on the right arm, the hand holding the edge of the pannel. At the instant at which she was setting the second foot on the ground, the horse made a motion which carried the right arm backward with some violence, and caused acute pain. Madame V. did not fall.

Next morning, Madame V. observed that the right shoulder was slightly swelled, painful on pressure, and especially on moving the arm; but as, during repose, pain was only little felt, the patient only confined the arm to inactivity.

In this state matters continued for five months, Madame V. not using her arm, and feeling only unfrequent and not acute pains. But in the month of November, when the patient began to use her arm, internal pains were felt; and very soon, by the continuance of the exercise, they became violent and continued. The movements attempted, nevertheless, were of small extent, and not forced.

At this time a practitioner who was consulted recommended repose of the member, oleaginous frictions, and poultices. These remedies were unavailing; the patient suffered severely and incessantly; fever more or less distinct appeared; and almost all the nights were spent without sleep.

Towards the end of November, a female somnambulist recommended constant and uninterrupted applications of recent cow-dung. Under the use of this cataplasin, or in the lapse of time, the pains were at the end of fifteen days alleviated, and the patient ceased to suffer,—only, however, providing the limb was left in complete rest.

About the end of January 1846, M. Marjolin recommended graduated movements and aromatised douches. Of the douches, nine were taken; but they gave rise, during administration, to great constraint in breathing, and to symptoms of cerebral congestion with headach, which was continued for several hours. The method producing no improvement was abandoned.

In the month of March, another practitioner prescribed purgatives, and caused the shoulder to be rubbed with croton-oil liniment. This was followed by violent inflammation and very acute pains, without any change in the movements.

In April, the patient subjected herself to the manipulations of the champooing woman at Chatillon. This person, at the first meeting, carried the arm upwards suddenly and violently, and recommended the patient to return at the end of five days. The forced movement effected by the champooer caused intense pain, which continued during the subsequent days, while the nights were sleepless and disturbed by almost constant fever. Notwithstanding the bad results of this proceeding, Madame V. kept the appointment; but at this time the champooer was unwilling to exercise any manipulation, assigning as reason that the patient was too feeble, and that she might conveniently continue as she was. The meeting was adjourned for eight days. At the end of this time a new forced movement, less violent than the first, was performed by the champooer, and repeated three times, after intervals of some days. The result of this treatment was deplorable. The joint became hot, red, swelled, and painful; the slightest movement forced cries from the patient. These symptoms subsided only at the end of three weeks.

In May, in pursuance of the result of a consultation, Madame V. went to Trivoli, in order to have cold douches directed to the diseased shoulder. Fifteen douches procured no modification in the movements, but rendered the joint once more swelled, red, and painful.

M. Roux, whose advice was next solicited, after examining the joint, and hearing the long list of means unavailingly employed, advised that the



patient should refrain from all treatment, assigning as reason, "You have an ankylosis already of long standing; it is probably an enemy for life; but, under all circumstances, it is necessary at first that you should refrain from movements."

This advice Madame V. followed. In order to re-establish her general health, which had been long feeble and greatly shattered by pain and treatment continued for twelve months, she resolved, further, to spend the summer in the country, and she came to reside at Bellevue. There desirous to get cured, and in the hope of deriving advantage from the neighbourhood of a medical establishment, the good effects of which she had heard commended, she, on the 20th June 1846, consulted M. Fleury, who then found the shoulder in the following state:—

The right shoulder had retained its normal figure; its volume was diminished in consequence of considerable atrophy of the deltoid muscle; there was neither redness nor swelling, but the skin was obviously hot over the articulation. The movements were very much limited, almost extinct. The arm could be carried forwards and backwards, but every movement requiring elevation was impracticable; thus the arm could be only very feebly raised outwards, and not unless accompanied by the scapula. The fore-arm being laid on the chest, the hand with difficulty reached the left breast; behind, the hand did not reach the line of the vertebral column; above, it could not touch the tip of the ear, unless the head and trunk were greatly inclined. The result of this state was, that most of the habitual acts of life were completely impossible; the patient could neither lace herself, dress herself, nor put on a head-dress, nor could she lay hold of any object placed in an elevated position. When the patient attempted movements within the limits now specified, she felt in the joint acute pains, which were occasionally protracted for thirty-six or forty-eight hours. When the arm was hanging, she could carry a light object without much suffering. Forced movements, however slight, caused intolerable suffering.

The general health was bad; the digestion capricious, and attended with pain; constipation was habitual and obstinate. The patient was much emaciated; the nervous system much shattered. The slightest painful mental emotion forces tears, and throws her into melancholy.

M. F. felt it his duty to explain that the cure was uncertain,—that under all circumstances the treatment would be protracted,—that cold douches alone would be inadequate, and that it would be requisite to conjoin with them forced movements. The patient promised patience and fortitude.

The first indication was to subdue the irritation existing in the joint, in order to render practicable the spontaneous and artificial movements. With this intention, he ordered the patient to keep the right arm in a state of perfect quietude, while cold water was employed as a sedative agent in the following manner:—Twice daily the patient received on the shoulder, for the space of ten minutes, a cloth-douche, and during the night, the joint was constantly covered by a wet cloth frequently renewed.

By the 2d July, the patient felt no pain in the joint; spontaneous movements performed without effort no longer caused suffering, and slight forced movements were easily endured.

The cold water was continued to be applied in the same manner; and the patient was recommended to attempt daily only those movements which she could perform without causing pain.

These spontaneous movements induced at first some articular pains; but these readily disappeared under the application of cold water compresses. By the 15th of July, the patient was able, without suffering, to move the arm within the limits already specified. M. F. now added forced movements in the following manner:—The patient being seated on a chair, with the fore-arm bent on the arm, and the scapula fixed with the left hand and right knee, he held the elbow with the right hand, and by slow

and graduated movement, he carried the humerus successively forward, backward, outwards, and upward. This was done twice daily.

At first these movements caused acute pain; but this was speedily allayed by the use of a cloth douche applied immediately, and by compresses applied for several hours after each attempt.

By the 1st of August, the spontaneous movements had become more extended, and were by no means painful. The patient exercised the arm most of the day. In the garden was suspended a rope with knots very near each other; and to this was fitted a leathern handle having moveable pieces of lead, so as to allow of the weight which was to be raised being gradually increased. Forced movements, increased in intensity, were well borne by the patient. For the cloth-douches were substituted a very forcible douche three centimetres in diameter, directed for the space of five minutes on the ankylosed joint; and, in order to avoid chilling of the person, Madame V. took a general shower bath. This treatment was followed by only a few fits of pain, which were soon abated by the use of wet compresses applied during the intervals or in the night.

By the 25th of August, three knots on the rope had been reached, and a weight of one kilogramme was easily raised. The hand reached without difficulty the left shoulder; behind, the lower angle of the scapula; above, the tip of the ear.

By the 25th of September, during the interval between last date and which time the same treatment was continued, pains had entirely disappeared; the patient raised and held for some time a weight of two kilogrammes; the movements forward and backward had attained nearly their normal extent; when the arm was carried outwards and upwards, the movement was almost equally complete; but the patient was unable to keep the arm in this position,—a circumstance owing manifestly to feebleness of the deltoid muscle, which had not recovered its natural size, and the fibres of which did not contract energetically. Four knots on the rope had nevertheless been attained.

Moveable douches and shower douches were now directed on the deltoid muscle.

By the 15th of October the cure was complete. The movements were as extensive, as easy, and as favourable as could be desired. The deltoid muscle had recovered its natural size and contractility. Madame V. used her arm as before the accident. Under the use of the shower baths, the general health was greatly improved. Appetite became active; digestion good; constipation disappeared; all the functions were performed regularly; and emaciation gave place to plumpness. At the 1st of June 1848, this satisfactory state continued, without having been once interrupted.

In this case, M. F. observes, that the disease had continued for one year; the loss of movements was almost complete. A cure could be effected only by the use of forced movements, which, on the other hand, were impracticable by reason of the local suffering and the symptoms of general reaction to which they gave rise. It was quite indispensable to refrain, as M. Roux recommended, from every new movement. Emollients and revellents (counter-irritants) were unable to subdue the increased sensibility of which the joint was the seat.

In this condition, the systematic employment of cold water as a sedative was of great service. It removed the spontaneous pains and those which were excited by motion; and it placed the articulation in that state in which forced movements became not only possible but easy, and certainly less painful than they were in ordinary circumstances, in subjects the most favourably situate in this respect.

When this end was accomplished, a change in the mode of applying the cold water, so as to substitute an exciting action for a sedative one, the douches became a powerful adjuvant to the forced movements. They

modified the vitality of the muscular and fibrous tissues; they restored to its normal state the deltoid, partly atrophied and paralyzed; and they rendered prompt and complete a cure, which, without them, must at least have been protracted, and yet might not have been so satisfactory.

In the next case, the twofold action of the cold douches, M. F. argues is shown with still greater precision and effect.

4. Madame A., resident at Paris, aged 36, of small stature, slender constitution, and well-marked nervous temperament, menstruated at 14, and since that period has suffered from profuse leucorrhœa. Her health was always good, excepting some wandering rheumatic pains, ascribed to habitual residence in a very damp apartment, looking to the north, and scantily admitting the rays of light.

In December 1842, Madame A., by the advice of a physician, began to employ, for the removal of the leucorrhœal discharge, astringent injections, which were continued daily and without interruption for five months.

One day in April 1843, immediately after an injection, the patient felt all at once at the vertex a sensation of heat and pressure, accompanied with giddiness, redness of the face, and tendency to swooning. These symptoms were continued for fifteen minutes, and then ceased suddenly, the sensation felt at the vertex passing then toward the nose, where it eventually disappeared. These attacks, which are termed nervous, took place daily, unless the attention of the patient was forcibly diverted, in which case they were sometimes wanting.

At the beginning of May, the gait became unsteady and tottering. The patient thought she was going to stagger and fall forward. This fear, which, however, was not justified by fact, became so great, that Madame A. dared no longer walk in her apartment without being supported. The unsteadiness of gait was greatly aggravated by any considerable mental emotion.

At the close of the month of May, Madame A. felt, especially during the night, very acute ophthalmic pains. She could not open the eyelids without suffering; and these coverings appeared to be in contact with a dry, rough surface. The palpebral mucous membrane was the seat of a thick viscid secretion, by which, every morning, the eyelids were agglutinated. Vision was disturbed and impaired.

In July, the eyes returned to their normal state, excepting that the sight remained feeble; but the symptoms, of which they were the seat, were followed by very acute pains affecting the temporo-maxillary articulations. These pains were aggravated by mastication, which became almost impracticable. They abated at the end of three days, but the motions of the lower jaw continued difficult and very limited.

In the month of August, sudden pains of short duration, but very intense, were felt in different parts of the body, sometimes in the muscles, sometimes in the articulations; these were unattended by redness or swelling.

In December, the patient felt, one day on awaking, very acute pains in the ankles. When she placed the feet on the ground, it was as if thousands of needles entered the parts. Three or four days after, the tibio-tarsal articulations became painful.

In January 1844, Madame A. applied for the empirical assistance of S. B., who prescribed a pisan of unknown composition, subjected the patient to regimen, and employed hot affusions, followed by frictions with hartshorn. This treatment was continued for five months, without inducing the smallest amelioration. For four months after, no treatment was employed.

In October, the state of the patient was greatly worse. The tibio-tarsal articulations were painful and swelled; the knees were also painful, swelled, irregularly mis-shapen, and walking was quite impracticable. Homœopa-

this treatment was employed, completely without effect; and it was given up at the end of six weeks.

In January 1845, acute pains were felt in the shoulders, elbows, and wrists; the motions were very painful, and they became more and more circumscribed.

In February, homœopathic treatment was resumed.

In the month of August, the knees were almost completely immovable; the legs were slightly inflated. In October, the disease attacked the vertebral column and the iliac bones. Homœopathic treatment was given up. The patient was obliged to keep, almost constantly, in bed. Between January and March 1846, the movements of the vertebral column and of the coxo-femoral articulations became more and more painful and limited. Between March and June, the patient was subjected to the use of chemical medicines. In October 1846, when M. Fleury was requested to treat the patient, she was in the following state:—

All movement had been for nearly one year impracticable. The patient continued in bed, observing the dorsal position, and a deep slough had been formed over the sacrum. To change position and perform the usual discharges, the patient had to employ the aid of two persons, and she uttered loud cries at every movement made, however slight.

She was extremely emaciated, which the patient ascribed to the locking of the jaws, which allowed her to swallow only some liquid aliments. The appetite was feeble. Constipation was obstinate; the skin dry, rough, harsh, of a dingy yellow colour; the countenance much changed. The external habit was analogous to that of individuals with well-marked saturnine cachexia.

The jaws were so closed, that the lower teeth were situate before the upper; and only with the greatest difficulty and pain could some limited movements be performed by the lower jaw.

Movements of elevation were entirely abolished in the shoulder; the fore-arm could be only slightly inflected; but extension was incomplete, and the movements of pronation and supination were gone. The movements of the wrists were lost; the fingers, with the greatest possible inflection, were distant from the palm fifteen centimetres.

The thighs were a little inflected on the pelvis. Through the abdominal parietes were felt the *psosæ* muscles, contracted and rigid. The movements of the hip-joint were entirely gone.

The legs were forcibly inflected; the movements impracticable; the knees were so pressed against each other, that the inner surface was excoriated, and a cushion required to be interposed between them.

The movements of the feet were altogether gone.

The trunk was incurvated; the vertebral column forming an arch of a circle with the convexity behind; while the patient could neither stretch nor bend herself.

Compared with the great general emaciation, the articulations appeared very large; but no organic change was ascertained.

In a case of disease so general and severe, M. Fleury was desirous to discover the cause of this form of general ankylosis, of which only two or three examples are recorded by Samuel Cooper and M. Velpeau, and to learn the resources of therapeutics in a disorder so inveterate and obscure. To obtain some light upon both questions, he proposed a consultation with M. D. Ricord.

This physician was satisfied that movement was very nearly abolished in all the articulations. He thought that this general ankylosis ought to be chiefly, if not solely, ascribed to muscular contractions (*contractura*) of a rheumatic or neuralgic character; and he recommended as remedies, baths, poultices, and emollient and narcotic frictions. By the patient the objection was urged, that she had at several times already employed these mea-

tures, without deriving from them the slightest relief; and as she was aware of the cure effected upon Madame E. by means of cold water, she solicited the application of a similar mode of treatment. M. F., having small confidence in the influence of topical emollients and narcotics, willingly acceded to this proposition.

Treatment was commenced on the 15th November 1846, and consisted in general and rapid lotions by means of a large sponge dipped in cold water, continued five or six minutes at a time, and repeated three times daily. Reaction was promoted by two or three coverings of woollen kept on for one or two hours. Between the 15th November and the 1st December, the lotions, especially the first, were followed by much pain; the cold producing a disagreeable painful sensation, while reaction was established with difficulty. These inconveniences were gone by the 1st of December.

Between the 1st and 15th, the lotions were well endured; reaction was prompt; the skin was no longer so dry and wrinkled; the appetite was better, and the patient felt stronger.

Between the 15th December 1846, and the 15th January 1847, the general condition of the patient was much improved. The complexion was better; strength returned under the influence of appetite and sleep; spontaneous pains were seldom felt in the joints; when she was raised and turned in bed, she no longer underwent the acute sufferings which, two months previously, made her scream aloud.

Next day, she was packed in the wet sheet, enclosed within two woollen blankets; in this state she remained for two or three hours, and, when sweating was established, an abundant lotion was applied.

By the 15th of February 1847, the teeth were no longer locked against each other; but the patient could perform some slight movements by the jaw. The fingers were less stiff; the knees could be separated between two and three centimetres; and the use of the cushion was given up. During night, Madame A. no longer glides in bed so much; she lies sometimes on the side. By the aid of two persons, she gets out of bed, and, supporting herself on two chairs, stands for two or three minutes. The trunk and thighs were still inflected on the pelvis, and the legs on the thighs.

Treatment by cold water was continued by packing in the sheet and lotions in the morning, and occasionally the shower-bath during the day and in the evening, all February and March, and the first two weeks of April. On the 15th of March, the patient could descend from bed without assistance; on the 25th she could walk round her apartment by the aid of two persons; and on the 31st, she was able, by means of a cane, to walk a dozen of steps. Between this date and the 15th of April, amendment did not proceed so rapidly as formerly, and M. F. advised the patient to reside at Bellevue, in order that treatment more energetic might be pursued. This she did. On the 20th of June she was able to walk from her lodging to the establishment with the aid of two or three batlings. By the 20th September, under the use chiefly of the shower-bath or douche three times daily, she recovered the motions of the jaw, of the wrists, and of the limbs. The fingers continued touching the palm of the hand. The functions were in good state, and the patient was plump. On the 20th October, though the movements were not perfect, yet they were sufficiently free to perform the ordinary duties of life; and with this the patient remained satisfied, especially as forced movements caused pain. Treatment was accordingly given up, excepting the use of the shower-bath two or three times in the week.

From the facts now stated, M. F. deduces the following inferences:—

1. In certain instances of incomplete ankylosis, against which forced movements are useless or hurtful above all therapeutic agents known, the preference ought to be given to exciting cold douches, which exert a very

favourable influence in rendering active the capillary circulation and organic absorption, in modifying the vitality of the tissues, and in thereby restoring the extra-articular and intra-articular tissues to their physiological conditions.

2. In the instances of incomplete ankylosis, which imperatively demand the employment of forced movements, but in which these movements are impracticable by reason of pain or articular irritation, and the phenomena of general reaction to which they give rise, cold sedative douches, better and more speedily than any other therapeutic agent known, cause the disappearance of these symptoms, and permit the surgeon to have recourse to graduated movements.

3. In instances of incomplete ankylosis which demand the employment of forced movements, and in which these are practicable, a cure is effected always more promptly, and sometimes more completely, by combining the action of exciting cold *douches* with that of graduated movements.

It seems unnecessary to add any observations on the foregoing cases. It is not easy to say to what particular form of articular disease they belong. It may be to that of what is termed capsular or synovial rheumatism. They deserve attention, because they show, whatever views as to their pathological character be entertained, the effects of the systematic use of the cold water and shower-bath. A case somewhat similar is given by Dr Gully in his work on the Cold Water Cure, in which, however, the *ankylosis* affected one knee-joint.

### III. MEDICAL PATHOLOGY.

*On the Contrast between Delirium Tremens and Inflammation of the Brain, as regards the quantity of Phosphoric Acid excreted by the Kidneys.* By H. BENGE JONES, M. A., F. R. S., &c. (Medico-Chirurgical Transactions, vol. xxx.. Lond., 1847.)—It is well known, that to distinguish between the symptoms of delirium tremens and those attending inflammation of the brain, is a very difficult task. Yet the diagnosis is so important in a practical point of view, that it is extremely desirable to have the means of forming a correct diagnosis. If the observations of Dr Jones be well-founded, they promise to contribute to this object.

Phosphoric acid is excreted from the system in combination with the earths and with the alkalis. If lime or magnesia be present in sufficient quantity, the whole of the phosphoric acid is precipitated in the addition of ammonia, in combination with these earths; but if an insufficient quantity of earthy matter be present, the excess of phosphoric acid remains combined with potash or soda. Usually the lime and magnesia excreted by the kidneys are not equal to combine with above one-tenth part, perhaps, of the phosphoric acid in the urine. From one-fourth to one-eighteenth will be earthy phosphate, and the other three-fourths to seventeen-eightieths alkaline phosphate. Hence the variations in the quantity of earthy phosphate precipitated by ammonia, will usually depend on the quantity of lime or magnesia passing through the system; and the sum of the alkaline and earthy phosphates must be determined, if we desire to arrive at a knowledge of the quantity of phosphoric acid passing out of the system. In an inquiry\* into the highest amount of phosphates excreted, it might have been anticipated that the determination of the lowest limit would be of equal interest with that of the highest; but very many analyses were made before the diminution of the phosphates in some cases of disease was made evident. Dr Jones finds, that in certain cases of delirium tremens the phosphates excreted are in very small quantity. In one instance, which is regarded as the lowest, the urine, specific gravity 1017·9, gave only 9·6 per 1000 parts.

\* For the mode of analysis see Philosophical Transactions for 1845 and 1846.

On the other hand, in cases of inflammation of the brain, the amount of phosphates is more or less increased; thus, in one case of inflammation of the brain, which is taken as the highest, urine of sp. g. 1031·1 gave 13·38 per 1000 parts, which is more than 200 times more than in the case of delirium tremens.

In general, in cases of delirium tremens, the urine is extremely scanty, being in some cases not more than four or six ounces in twelve or even twenty-four hours. It is also in general thick, scanty, and high coloured; but when examined, it is found either to contain little or no phosphoric acid, either free or combined. During the acute stage of three cases of delirium tremens given in this paper, the average amount of phosphates is only ·67 per 1000 parts of urine of density 1020. Conversely, in three cases of symptoms of inflammation of the brain, the average amount of phosphates is 8·26 per 1000 parts of urine, at density 1025. In other words, the average amount of phosphates excreted during inflammation of the brain, is above twelve times greater than it is during delirium tremens. From this it may be inferred, that if upon analyzing the urine is doubtful cases, the amount of phosphates is found very small, the case may be believed to be one of delirium tremens.

The only difficulty is this, that not in every case of delirium tremens does this diminution of phosphates take place, and not in every case of inflammation of the brain are the phosphates increased.

In speculating upon the physiologico-pathological explanation of this peculiarity, Dr Jones thinks that, whereas in the healthy state a portion of the phosphoric acid excreted results from the action of the inspired oxygen on the phosphorus of the brain, in delirium tremens this formation of phosphoric acid is greatly diminished or suspended; while during inflammation of the brain the formation of phosphoric acid is abnormally increased.

It is further important to remark, that if food can be taken in delirium tremens, the food furnishes phosphates, which hinders the diminution from being apparent, and it is also an indication of convalescence that food is taken, and the phosphates are increased. On the other hand, during inflammation of the brain, food is neither desired nor digested, and until the phosphates are reduced in quantity to near the natural standard, it would be improper to believe the brain in a healthy state.

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PART I.  
ORIGINAL COMMUNICATIONS.

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ART. I.—*Statistics, &c. of Glasgow Royal Infirmary, 1848.* By  
JOHN CHARLES STEELE, M.D., Superintendent of the Infirmary.

THE annual vital statistics of hospitals present for the most part, in their general outline and details, so striking a similarity of results and confirmation of previously-established inferences, that it may almost appear superfluous to attempt to deduce from the records of any single year, fresh subjects of novelty and importance, that might prove interesting to the medical inquirer. Nor are the data thus afforded to be received as altogether free from the imputation of fallacy. In the construction of every extensive disease list, various maladies are taken for granted, and not a few too frequently passed over, from the fact of their classification interfering materially with a well-looking pathological table. Thus, no more common error is committed in the preparation of these returns, and one to which all concerned in their construction must plead more or less guilty, than to affix to each patient some particular malady for which he had been treated, without making allowance for co-existing complications, which so frequently attend the progress of disease, and have a direct influence on the results of practice. Again, in subjecting to statistical analysis those series of affections which are rarely met with either in hospitals or elsewhere, the data which we possess are

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insufficient to warrant any practical deduction; while in other diseases of uncertain seat, the difficulties attending diagnosis are such, that even in the practice of the most erudite, a mere conjecture is frequently all that can be arrived at during life, and the true nature of the malady fails to be elicited, save by the aid of the scalpel after death. With the exception of the data furnished by the more usually prevalent affections of the respiratory organs, by the recorded results of practice in zymotic diseases, and the more frequent surgical operations, there are few other sources of information from which deductions could be made that are free from the risk of being overturned by subsequent experience, or found upon closer scrutiny to be opposed to truth.

As the results of a numerical analysis of each separate disease are in a great measure fruitless, and but ill requite the serious labour entailed on the statist, we are induced to adopt a more general plan in the construction of the present report, than that previously followed in this Journal for the four preceding years; and to confine our observations to those general results which are most comprehensive and therefore least likely to mislead. It is not meant on this account to attach less value to the minute details of statistical investigation. On the contrary, the foregoing observations afford in themselves negative evidence of the importance of attending to accuracy in all branches of numerical investigation, and of adhering to some uniform system for the purpose of comparison with other and similar sources of information.

Of late years this method of reasoning has deservedly obtained a paramount bearing in the solution of many important questions connected with pathology and hygiene. In practical surgery it claims precedence over all other doctrines for ascertaining the true amount of success attendant on different operations; and, while it condemns almost *in toto* the performance of one set, it exhibits also the trivial amount of danger attending others, formerly considered as the most hazardous. The opportunities afforded by hospital practice are superior to all other sources from which we seek to obtain information. A larger number of diseases are annually treated in each establishment than can possibly fall to the lot of any single practitioner. Faithful, and in many instances comprehensive, records of the cases are kept, which are generally open to the medical public for inspection. A careful transcript of the results can thus be readily obtained, and attempts to make the practice more successful than it justly deserves, are by these measures entirely obviated.

Almost every town in Scotland in which the population exceeds 12000 is now possessed of an hospital for the relief of its diseased poor, and is conducted by a respectable board of management, who annually furnish to the local public a full statement of their

proceedings, along with disease tables compiled from the records of cases occurring during the year. These appendages to the ordinary reports, although on the whole very accurately and comprehensively drawn up, are exceedingly wanting in uniformity of system, so that many diseases,—we would instance particularly the various types of fever,—are enumerated in such a way that it is morally impossible to define with precision the extent and prevalence of any peculiarity. It would facilitate much the progress of statistical inquiry if one uniform system was adopted by hospital officials in the preparation of disease lists, and less attention was paid to suit the returns for the inspection of the non-medical public, with whom they can at best only gratify morbid curiosity without being conducive of any ulterior advantage. A large amount of information, collected from different sources and extending over a series of years, comprising the aggregate results of a multiplied series of facts, would thus be obtained; and much valuable matter, which at present is almost lost sight of, could be turned to good account in ascertaining the relative mortality of disease in different hospital districts, besides throwing light on the sanitary conditions which lead to the same.

In the report for 1847 we adverted to the extensive and fatal visitation of fever to which Glasgow, in common with the rest of the country, was subjected at the period, and which compelled the authorities to have recourse to a large amount of auxiliary accommodation, as that afforded by the Royal Infirmary was found inadequate to meet the emergency. The spring of the past year happily witnessed the gradual and steady decline of the epidemic, and gave reasonable hopes for the future continuance of a better state of things. The temporary accommodation in connection with the infirmary was dispensed with on the 24th of February; and the number of fever cases gradually declining throughout out the city and suburbs as the year advanced, the parochial authorities closed the auxiliary hospitals they had instituted, and sent their diseased poor, as formerly, to the infirmary. Notwithstanding the extreme prevalence of epidemic catarrh in the early part of the year, and of scarlatina during the autumn months, comparatively few cases of these diseases are appended to our returns, arising partly from the fact of both affections prevailing more extensively among the upper classes of the community than among those who generally seek refuge in hospital, and partly also from the comparative mildness of the symptoms in the former disease,—a circumstance always of considerable weight with the labouring class before they are necessitated to solicit medical advice. After the month of May the number of fever cases remained stationary or nearly so throughout the remainder of the year, while, during the whole period now

under consideration, medical and surgical cases kept rather below an average, and present likewise a proportionately diminished amount of mortality. The calm, however, which succeeded the severe storm of epidemic visitation, was destined to be but of limited duration. Rumours of an approaching pestilence far more appalling than its forerunner had already alarmed the public mind, and constrained the guardians of the public health to assume every necessary precaution for the purpose of grappling with the dreaded malady. Subsequent experience soon proved the necessity as well as the comparative futility of the best directed and most philanthropic measures on their part. Epidemic cholera first visited Glasgow on the 4th of November, previous to which time arrangements were made by the managers to accommodate as many cases as possible within the infirmary; and accordingly the fever house in connection with the institution was set apart solely for their reception, being nominally opened on the 27th of November, the period when the first cases were brought for admission. As the epidemic did not become prevalent till near the close of the year, and as other hospitals were established by the parochial authorities, our cases are necessarily limited in number, and present but a small fraction of the total treated during the year. The following abstract will exhibit the general movement in hospital during 1848 :—

Remaining 1st January 1848 from former year,	366
Admitted since to 1st January 1849,	3770
<b>Total under treatment during the year,</b>	<b>4136</b>
Dismissed as cured,	2663
Relieved, &c.,	636
Died,	562
Remaining 1st January 1849,	275
	<b>4136</b>
<b>Medical and Surgical Hospital.</b>	<b>Fever Hospital.</b>
Remaining 1st January 1848, 172	Remaining 1st January 1848, 194
Admitted since, 2184	Admitted since, 1513
<b>Total under treatment, 2356</b>	<b>Total under treatment, 1707</b>
Dismissed as cured, 1266	Dismissed as cured, 1386
Relieved, &c., 636	Died, 250
Died, 275	Remaining 1st Jan. 1849, 71
Remaining 1st Jan. 1849, 179	<b>1707</b>
<b>2356</b>	
<b>Cholera Hospital, opened 27th November 1848.</b>	
Admitted to 31st December inclusive,	73
Cured,	11
Died,	37
Remaining in hospital 1st January 1849,	25
	<b>73</b>

A general outline of the diseases and injuries treated in the medical and surgical department is furnished by the next table, arranged under 13 systems, for the sake of brevity and illustration.

	Cases.	Cured.	Reliev., &c.	Died.
1. Fevers, . . . . .	33	17	14	2
2. Diseases of the nervous system, . . . . .	133	62	59	12
3. " " circulatory system, . . . . .	51	22	21	8
4. " " respiratory system, . . . . .	261	105	96	60
5. " " genito-urinary syst., . . . . .	247	138	90	19
6. " " gland. & secret. syst., . . . . .	101	49	45	7
7. " " integumentary syst., . . . . .	158	116	37	5
8. " " osseous system, . . . . .	300	216	45	39
9. " " artic. & fibrous syst., . . . . .	202	115	76	11
10. " " chylipoietic system, . . . . .	136	94	22	20
11. " " eye, ear, mouth, throat, . . . . .	34	25	7	2
12. Dropsies, . . . . .	121	32	52	37
13. Miscellaneous diseases, . . . . .	400	275	72	53
Total, . . . . .	2177	1266	636	275

1. **FEVERS.**—Cases of fever not unfrequently find their way into the medical wards of the hospital, although the standing orders forbid their being placed in connection with ordinary medical patients. In the majority the primary disease had been obscured at the period of admission, or it had supervened when the patients were under treatment for some other disease,—the total number annually arranged in this way varying in amount with the presence or non-prevalence of an epidemic. In the course of the past year, 33 cases are reported as having occurred under the above circumstances, manifesting the following types:—Typhus, 9 cases, all sent to the fever hospital; synochus and relapsing synocha, 7, 3 dismissed cured, 3 sent to fever wards, and 1 died from the severe gastro-hepatic complication which was formerly so characteristic of this type of fever, but has latterly almost entirely disappeared; 4 cases of dothineritis; 3 of small-pox; 2 of ague; and 7 of slight pyrexia, comprise the remainder of the patients dismissed. The remaining death was occasioned by scarlatina, and occurred in the case of a female admitted with necrosed *tibia*, who fell a victim to the malignant form of the exanthem after a short residence in the house. The column headed *relieved* is properly excluded in the classification of our fever tables, being deemed unnecessary, but is made use of in the above enumeration to indicate the number of individuals affected with fever who were removed to the ordinary fever accommodation; as, in the event of the disease being considered contagious, the danger to be apprehended from retaining the affected in contiguity with those not similarly affected is very great, and more than counterbalances the supposed advantages said to attend the mixed system.

Early segregation of those contagiously affected, so far as it is

consistent with the safety of the patients, has been universally the practice of the Glasgow hospital, both in ordinary seasons and during the progress of epidemics; and, notwithstanding the mortality from exposure among the officials and assistants on the last mentioned occasions, has always been heavy, and a matter of serious reflection and regret to all concerned in the management, the arrangement is rendered unavoidable from the large numbers presenting themselves for treatment. The virulence of the contagious principle is, moreover, well known to rise and fall in intensity with the increase and decrease of the disease; and the dangers to be apprehended in seasons of trivial prevalence of fever are so far diminished, that it is a rare occurrence to meet with a case of fever among the attendants on these occasions; consequently, little benefit could accrue by a deviation from an established rule, which had been found to work as well in practice as in theory. No better illustration of this fact can be furnished than the following, embracing the practice of the hospital for the last two years and a-half. Dr Orr, in his paper on the epidemic fever of 1847, No. 175, when alluding to the mortality among the parties engaged in attendance on the sick, mentions the fact of forty of the officials connected with the infirmary having been attacked. But since that time to the present, extending over a period of fifteen months, we have not had a single case of fever among the attendants, although the number of cases of this description resident in hospital at any one time was never below 50, and the estimated average number 85. We have been induced to digress thus far from the general tenor of our remarks, as the subject has of late given rise to a good deal of discussion in some quarters, nor does there seem a probability of its being soon brought to a final adjustment.

2. DISEASES OF THE NERVOUS SYSTEM.—The numbers annexed to this head indicate about the average amount of nervous diseases annually treated in hospital, and are composed chiefly of cases of general and partial paralysis, along with a miscellaneous variety of hysterical and neuralgic affections. The deaths reported were 12 in number, 3 of which are ascribed to hemiplegia, 2 to hydrocephalus, 2 to tetanus, 1 to delirium tremens, and the remaining 4 to chronic diseases of the brain.

Instances of delirium tremens are of rare occurrence in the Infirmary as compared with the returns of other hospitals. During the past year, only 4 cases are reported proving fatal, and the total number for the last four years does not amount to more than 20, of which 7 have died; while in the last published statistics of the Edinburgh Infirmary, I find no fewer than 70 cases for one year, with 23 of a mortality. As there is no

positive regulation forbidding the admission of cases of delirium tremens to the Glasgow hospital, we presume that this statistical discrepancy between the two principal hospitals in Scotland, which in most respects bear a well-marked similarity, must depend more on the extra accommodation set apart for the purpose in Edinburgh than upon any inference we can draw in favour of the superior sobriety of our working population.

Cases of tetanus and pseudo-tetanus recorded were four in number, two having been affected with the acute and well-marked form of the malady, whose cases proved fatal, and two with the milder form recovering. The latter occurred under Mr Lyon's care, and presented some peculiarities in the symptoms worthy of notice. The first of these, to which I may advert, is that of a young woman, Anne Smoulin, aged 19, who figures in the records at three separate intervals during the year, as having been treated for the anomalously different affections of tetanus, primary syphilis, and typhus fever, and who presented herself a fourth time at the hospital with secondary symptoms, but was refused admission. This patient ascribed the cause of the nervous disease to the extraction of a molar tooth; and the symptoms, which were chiefly those of trismus, were at first readily relieved by the administration of chloroform. Shortly after admission, the anæsthetic agent failed in its relaxing influence, and recourse was had to strong purgatives and antispasmodics with apparently little benefit, as, in a report taken on the thirteenth day of her residence, the symptoms are stated to continue without abatement. By the persevering use of the remedies, separation of the jaws was at length partially effected, and the patient was ultimately dismissed well, after a stay of thirty-five days. Little doubt was entertained that the affection was of an entirely hysterical character in this patient, as she was known to suffer from catamenial irregularities for some time previous to admission. But the obstinacy of the symptoms, as well as the cause assigned for their appearance, show to what extent traumatic trismus may be simulated by the spurious form of the disease.

The other instance occurred in a young lad aged 17, who had sustained an extensive burn on the upper part of the trunk, including both arms, for which he was treated without any untoward symptom appearing for a period of seventeen days subsequent to admission. During the interval, the ulcerations caused by sloughing of the integuments were healing kindly, and the patient was otherwise proceeding favourably; but, at the time alluded to, he became affected with spasmodic rigidity of the maxillary muscles, with inability to move the jaw. On the day following, the muscular spasms extended to the neck and abdominal muscles, and the patient remained in a state of partial opisthotonos

for twenty-three days afterwards, with but little intermission of any of the symptoms, notwithstanding the active treatment to which he was subjected. As the ulcers proceeded to cicatrization, the stiffness and rigidity gradually lessened, and he was dismissed well after a seventy-seven days' residence.

3. CIRCULATORY SYSTEM.—The diseases classed under this head are limited when compared with the others; and if we withdraw from the numbers specified in the table, cases of *morbus cordis* and scorbutus, we nearly exhaust the system. Diseases of the heart, exclusive of cardiac dropsy, amounted in all to 28 cases, 6 of which were reported cured, 11 relieved, 5 dismissed with advice or in *statu quo*, while 6 died. There are few points connected with medical statistics, on which so little reliance can be placed, as the returns from the present source. Fully sensible of the defects attending a systematic classification of cardiac diseases, most statisticians agree in grouping them under one head, as the inaccuracies attending attempts at a minute subdivision would be even more unsatisfactory than the present system. The same remark applies to the returns furnished from cardiac dropsy, which indeed should be reckoned a mere symptom of the former, and not a disease *per se*, and might with propriety have been classed under the head of *morbus cordis*. But as an opposite practice has been formerly followed, for the sake of uniformity, we have adhered to the same in the present report.

Cases of aneurism were four in number,—three aortic, two of which were thoracic, and one implicating the abdominal portion of the artery, all dismissed with partial relief. The remaining case occurred in an old man affected with femoral aneurism, who, refusing to submit to the usual operation, was dismissed as an irregular case. An instance of death, referable to the present group, is instructive, as it shows the unforeseen casualties which may attend what are commonly deemed minor operations in surgery. A child, aged 20 months, was admitted with a *nævus maternus*, situate immediately over the left orbital region. Prior to admission, the growth had increased to nearly the size of a walnut, and pressure proving unsuccessful after repeated trials, recourse was at last had to ligature. The case proceeded favourably for two days after operation, subsequent to which time, comatose symptoms made their appearance, accompanied with slight convulsions, and terminating in death. No autopsy was permitted to be made. But there was little doubt, from the nature of the symptoms and contiguity of the structures implicated, that congestion of the brain had ensued after operation, and was the proximate cause of the fatal issue.

4. RESPIRATORY SYSTEM.—Diseases of the respiratory or-

gans afford fuller materials for statistical analysis than the classes already enumerated, inasmuch as the data which they furnish are more comprehensive, and the diseases themselves, from their frequency and well-known characters, are less liable to be confounded with others. The most formidable, as it is also the most numerous class of affections in the group, consists of cases of pulmonary consumption, which for the year amount to 99, 63 having been dismissed the house in various conditions, from the temporary return to health till the near approach of death, and the remaining 36 terminated fatally within the hospital. Of the other diseases uniting to swell the list, bronchitis furnishes 92 cases, of which 66 were cured, 18 relieved chiefly of a chronic character, and 8 died. Pneumonia affords 24 cases,—16 recoveries, 3 relieved, 5 deaths; and pleurisy 11 cases, with 7 recoveries, 1 relieved, and 3 deaths.

In the returns from phthisical cases, it appears to have been the established rule in the Glasgow hospital not to classify any such under the column set apart for the cases dismissed as cured, even when the apparent return to health was such as to warrant that conclusion. It is perfectly obvious, however, that in a disease of such an unmanageable nature as pulmonary phthisis, little reliance will be placed upon data prematurely furnished from the results of a few weeks' residence of the patients in hospital; and we have consequently adhered to previous custom in the preparation of the present returns, being at the same time fully aware that the flattering results of practice in one or two of the cases annually treated, are sufficient to justify us in bearing testimony to the accomplishment of what may well be ranked among the most important objects of pathology.

We have less reluctance in adverting to those cases which come under the category of relieved, as there are many such annually reported, even when the disease is considerably advanced in its progress, where much relief is obtained by a short residence in hospital. Of the above 99 cases, 57 were recorded as having undergone more or less improvement; 4 were dismissed at their own request; 1 for misconduct; and, strange enough, only 1 alleged as incurable. The majority of the cases relieved in the Royal Infirmary, and, we have reason to believe, a large proportion of cases dismissed with similar results from other institutions, present the same following characteristic features, explanatory of the benefits they obtain from treatment. Consumptive individuals of the poorer classes, who have previously enjoyed but indifferently good health, after exposure to cold or some other exciting or debilitating cause, present themselves for admission, frequently with urgent symptoms of extreme pulmonary congestion, or, it may be, with a considerable amount of acute or subacute pulmonic inflam-



mation. After a short stay in the house, where they are, in the meanwhile, submitted to mild antiphlogistic treatment, the symptoms become very much abated, and the patients are dismissed with the recommendation to betake themselves to the country, before the primary mischief, which may as yet be only in its incipient stage, has time to produce disorganization in the textures implicated. It is in a great measure owing to these conditions, fully explained by Dr Latham, and by him appropriately termed mixed phthisis, that we are to ascribe the partial success attending the treatment of the disease in hospitals, although the results are influenced to a large extent by many fortuitous causes, such as locality, facility of admission to hospital, length of residence, &c.

The following table, constructed from the most recent published reports of six hospitals in Scotland, will show the relative results of each, and how various causes tend to modify the same :—

Infirmaries.	Cases.	Cured.	Relieved.	Other grounds.	Died.	Mortality per cent.
Edinburgh Infirmary,	145	5	46	15	79	54.48
Glasgow Infirmary,	99	...	57	6	36	37.37
Aberdeen Infirmary,	27	2	11	6	8	29.63
Dundee Infirmary,	10	3	...	...	7	70.
Perth Infirmary,	19	...	6	...	13	68.42
Greenock Infirmary,	10	...	1	3	6	60.
Total,	310	10	121	30	149	48.64

Contrary to what we might, on first thoughts, anticipate, the mortality in the provincial hospitals is considerably higher than that of the Edinburgh and Glasgow infirmaries; the Aberdeen infirmary, for reasons which we do not pretend to explain, proving the only exception to the rule. A striking difference further is manifested by a comparison of the annual mortality returns from phthisis of the Edinburgh and Glasgow hospitals, arising, no doubt, from the longer stay of the patients in the former, and partly, we have reason to believe, from the admission of a larger number of hopeless and advanced cases of the disease. The average period of residence in the Edinburgh hospital appears, from the last statistics, to extend over 47 days; but, if we deduct from the total number the large percentage of cases terminating fatally, whose average residence is always much shorter than the others, we have little doubt that we would find the estimated stay of the relieved cases to come close upon 60 days. In the Glasgow hospital, on the other hand, the estimated residence of each patient does not exceed 29 days; and, as the mortality is some 17 per cent. less than in Edinburgh, the average stay of the relieved cases must consequently increase in a less proportion than the same cases in the last mentioned institution, and will probably be found not to exceed 35 or 36 days. But on referring to Dr

**Walshe's report** of the practice in the Brompton hospital\* for consumption, we find it stated, as a general rule, that the mean length of stay of the most favourable class of cases nearly doubles that in the least favourable, and that improvement was more common than the reverse, even when excavation of the lung existed on admission. It would appear to follow, therefore, that there are other causes besides the lengthened residence which lead to a disproportion in the amount of mortality in separate establishments; and there seems much reason for entertaining the supposition, that the cause of the discrepancy in the returns of the Edinburgh and Glasgow hospitals is occasioned as much by the admission of a larger number of dying cases in the former institution, as by the longer stay of the patients in hospital.

The propriety of admitting hopeless cases of pulmonary consumption into hospital has been the subject of much difference of opinion, and is frequently censured; and, were we to judge merely from the mortality attending the practice, there is no lack of evidence to convince us of the truth of the previous statement. But the position in life, the domestic habits of the sufferers, as well as the want of any other asylum to meet their case, all combine to render equal facility of admission the best and most justifiable practice; indeed, the only alternative left under existing circumstances.

It is much to be regretted, that, in some rural and acknowledged healthy district of Scotland, there has never been a separate asylum established whereby the consumptive poor might enjoy the advantages of removal to a purer air than can be found in a general hospital in a large city,—benefits which at present are, in a great measure, confined to the wealthier classes of the community. The results of the practice at the Brompton hospital fully testify to the immense advantages accruing from the maintenance of a separate establishment for the phthisical, and give encouragement to the idea of the establishment of a similar institution in Scotland, which would confer invaluable blessings upon a class of individuals hitherto strangers to everything pertaining to domestic comfort, and whose melancholy lot is well calculated to excite the sympathy and procure the support of the benevolent.

The influence which age and sex have exerted on the returns from phthisis for the past year is so analogous to the reports of former years, and the details so similar to those already published in your Journal, that we forbear repeating them in a separate form. The following table, deduced from the statistics of the last four years, will exhibit a more comprehensive groundwork for analysis:—

\* *Medico-Chirurgical Review*, No. 5.

Ages.	Total.	Dismissed		Died.		Mortality per cent.		Av. residence in days.	
	M. and F.	M.	F.	M.	F.	M.	F.	M.	F.
From 1 to 10	6	4	1	...	1	...	50	32	16
... 10 to 20	68	30	20	11	7	25·27	26·92	27	29
... 20 to 30	149	60	32	30	27	33	45·76	29	30
... 30 to 40	97	42	16	22	17	34·37	51·51	29	29
... 40 to 50	40	19	5	9	7	32·14	58·33	25	34
... 50 to 60	7	3	2	2	...	40	...	30	24
... 60 & upwards	1	...	...	1	...	100	...	10	...
Total,	368	158	76	75	59	32·14	43·70	28	29

From this summary of the more important results, it follows, that the decennial period from 20 to 30 furnishes by far the largest number of cases, and that the admissions decrease in such rapid proportion after this period of life as to leave comparatively few cases subsequent to the age of 50. Age itself, as judged of from the returns of the various decennial periods, does not appear to influence the mortality in any great degree, and the data in the preceding table, sufficiently comprehensive to warrant this deduction, are more to be relied on from the average residence of the two sexes in hospital being very nearly the same in either sex. A more remarkable difference is observable from an estimate of the mortality in the two sexes. Females exhibit an excess in mortality of about 11 per cent. over males, although the mean residence of such class in hospital is nearly equal. We must make allowance, however, as we did formerly in explanation of the results in the Edinburgh and Glasgow hospitals, for the mean residence of the cases dismissed, averaging much longer in that class which exhibits the highest amount of mortality, and *vice versa*; consequently, the stay of the more favourable class of female cases, reduced in the table to 29 days, will be found, *ceteris paribus*, to exceed considerably the mean residence of the males who are similarly circumstanced. But this is barely sufficient to account for the deviation, which appears confirmed by the results of experience in other hospitals, and there are doubtless other causes in operation of a more powerful nature than the preceding, to explain the fact. The want of sufficient data forbids us speculating too widely on the subject. But it is extremely probable that the smaller amount of mortality among males is attributable in a great measure to the greater frequency of the mixed cases of the disease among that class, the habits of the sex exposing them in a more particular manner to those exciting causes productive of inflammation. The shorter stay of the same parties may also be partly accounted for by their anxiety to return to their usual employment, more especially if they have

families depending on their exertions, in which case males must rest content with a smaller amount of benefit than will fall to the lot of the female sex who are placed in an analogous position. The proneness of the phthisical to contract early marriages, as alluded to in Dr Walshe's report, we have been able to confirm by examination and comparison of the records of non-phthisical cases along with the former, and find that the disposition exists much in the same ratio in both sexes.

We are not sure if any inference can be drawn from an analysis of the different occupations which the patients followed, that might assist in explaining the disposition of phthisis to affect one class of operatives over another. Among the males, labourers and weavers, and among females those engaged in factories, include by far the largest proportion of the cases admitted. But as these individuals represent the most populous class in a labouring population, it would be unfair to deduce any conclusion from this head, without first ascertaining the relative proportionate number of these parties in a given mass of population.

The form of the disease commonly known by the name of collier's phthisis, and attended with the infiltration of black matter into the substance of the lungs, is perhaps more frequently met with in the Glasgow hospital than in most other institutions. The smoky condition of the atmosphere in a district where large quantities of coals are consumed in various manufactories, and especially in iron foundries, and the contiguity of the mining districts, from whence a number of cases are annually sent, sufficiently account for the exposure of the patients to this species of carbonaceous inhalation. Again, any one conversant with the practice of the Edinburgh hospital will have remarked the large number of stone-masons annually treated for phthisis in that establishment; while in the Glasgow infirmary, on the other hand, the number of operatives of the same class admitted are extremely limited. This discrepancy is alleged to arise from a difference in the hardness of the workable materials in common use in either city.

5. DISEASES OF THE GENITO-URINARY SYSTEM.—Primary and secondary syphilitic cases comprise the most numerous class of affections arranged under the present group. Of the former, there are in all 23 cases, 20 of which were completely restored to health, and 3 relieved. Of the latter, there were 88 cases; 59 cured; 27 reported as relieved, or had a *statu quo* condition kept up during their hospital residences; and 2 terminated fatally, from severe internal complication. From motives of concealment, and partly also from the pecuniary fine exacted on admission, the number of primary syphilitic cases annually treated bears a marked disproportion when compared with the larger returns of the second-

ary cases. These formidable results, arising no doubt from a total neglect of all medical treatment in the early stages, seem to call in question the propriety of fining the delinquents in primary cases. Certainly the moral intentions thought to be inculcated by the continuance of the practice are more than counterbalanced by the host of physical evils consequent on neglect. *Diabetes mellitus*, examples of which disease have become of late years extremely common in the Glasgow hospital, furnishes 11 cases for the present report. Of these, 1 is reported as cured, 7 relieved, 2 dismissed with advice, and 1 case terminated fatally. The remaining diseases of the genito-urinary class are made up principally by a miscellaneous variety of uterine and urinary complaints, fistulæ of various characters, strictures and injuries of the generative organs; but the data are insufficient to justify a recital in this place.

#### 6. DISEASES OF THE GLANDULAR AND SECRETORY SYSTEM.

—Abscesses of various parts and of different degrees of severity, acute, chronic, circumscribed, and diffuse, along with the long list of specific diseases resulting from the strumous diathesis, nearly absorb the entire diseases of the present group. Its position, in any systematic classification, is thus rendered very equivocal from the mixed and anomalous characters of the affections concerned; and this is more particularly applicable to the case of scrofula, which exhibits itself in such a variety of forms. Many instances of the latter are, however, excluded from the present classification, on account of the well-marked characters of the symptoms in various cases justifying their separation, and entitling them to a more minute subdivision in the disease list. Abscesses alone furnish 57 cases; of which, 37 were cured, 14 relieved, and 6 proved fatal. Scrofula presents 32 cases; 9 dismissed as cured, 22 relieved, and 1 case terminating fatally. The death occurred in a boy, who had undergone amputation of the arm on account of strumous disease of the elbow-joint, and who was, in external respects, a perfect martyr to the diathesis. Death supervened a few weeks after operation, previous to which the opposite elbow had become very much involved, and hastened the fatal termination, which was also partly ascribed to colliquative diarrhœa. Necroscopic appearances in this case contradicted the supposition of internal disease, as the viscera of the chest and abdomen were found, on examination, to be entirely free from any scrofulous or tuberculous deposition. Among other returns from the present class, diseases of the glands, independent of suppuration, afford annually a small proportion of cases. The most prominent disease of this class is scirrhus mamma, of which 7 cases are reported, with the following results: 6 were dismissed with advice, or in a *statu quo* condition, in accordance with the non-operative interference now so

generally followed in the treatment of scirrhus ; and 1 was returned as cured, where, from the circumscribed condition of the tumour, and the good health of the patient, a favourable opportunity was presented for excision, and the patient was subsequently dismissed well.

7. DISEASES OF THE INTEGUMENTARY SYSTEM.—Under the integumentary system are included the various diseases of the skin and subjacent tissues, with the exception of the exanthematous eruptions diagnostic of the different types of fever already alluded to, System 1. Erysipelas, which, either in its idiopathic or traumatic form, is seldom absent from the wards of the hospital, forms as usual the bulk of the cases under consideration, there being no fewer than 58 returns from this source during the year. Of these, 50 were dismissed cured, 3 were relieved, and 5 died ; 3 of the deaths having arisen from the disease implicating the face and scalp, and 2 from its supervening upon injury. Exclusive of erysipelas, the other tegumentary affections, properly so called, amount in all to 84 ; of which, 58 were cured, 26 left the hospital more or less relieved ; none being dismissed as altogether incurable. *Psoriasis* and *porrigo capitis* comprise 35 of the above number, and were attended with the following similar results : of the former, 13 were cured and 5 relieved ; and of the latter, 12 were cured and 5 were relieved. From the obstinate and recurrent nature of the present class of diseases, the mean stay of the patients in hospital is considerably prolonged, averaging, over head, in the present returns, about thirty days.

8. INJURIES OF THE OSSEOUS SYSTEM.—Fractures of all degrees of severity, and a variety of affections peculiar to the osseous structure, comprise the numbers recorded under this head. The following classification of fractures will be observed almost to exhaust the group :—

Nature of Fracture.	Total.	Cured.	Relieved, &c.	Died.
Simple of upper extremity, . .	36	29	7	0
„ of lower extremity, . .	119	113	5	1
Compound of upper extremity, . .	19	11	3	5
„ of lower extremity, . .	41	16	3	22
Total, . . . .	215	169	18	28

Three deaths are reported from fracture of the skull, and one case was dismissed cured of the same casualty. Of fracture and injury of the spinal vertebræ there are 3 cases, of which 2 died and 1 recovered. As these cases were all the result of accident, and as many of them had afterwards to submit to operative interference, the causes leading to, and the operations incident on, the same, will be best seen by referring to the respective tables.

9. DISEASES OF THE ARTICULAR AND FIBROUS SYSTEM.—Cases of acute and chronic rheumatism, various injuries, dis-

locations and diseases of the joints, exclusive of scrofulous origin, unite to form the total treated under this head. Rheumatism furnishes 73 cases, of which 52 were cured, 20 relieved, chiefly chronic cases, and 1 case terminated fatally. Several of these were complicated with disease of the heart, but I regret much that the data I possess are not sufficiently comprehensive or accurate to warrant recital.

10. DISEASES OF THE CHYLOPOIETIC SYSTEM.—The diseases which fall to be enumerated under this group are necessarily of a very miscellaneous character, and afford comparatively limited data for statistical analysis. The most numerous class of affections are furnished from the returns of diarrhoea and dysentery; the former comprising 22 cases, of which 17 were cured, 1 relieved, and 4 died; the latter likewise including 22, of which 15 were cured, 1 was relieved, and 6 terminated fatally. Notwithstanding the epidemic prevalence of bowel complaint during the last months of the year, the returns from this source were not sensibly affected, and barely average one-half of the cases treated during 1847, the spring of which, it will be remembered, was peculiarly prolific in the production of intestinal affections. A very remarkable case of intestinal concretion, the result of pica or depraved appetite, is reported under this group, and presented the following features, for the particulars of which I am indebted to Dr Ritchie, under whose care the patient was placed:—

Isabella Williamson, aged 21, factory woman, admitted 4th October. The patient stated, that, for a short time prior to admission, she had been affected with severe and painful vomiting, accompanied with constipation of the bowels, and had suffered more or less from disordered menstruation since the period of puberty. On examination, the abdomen was discovered to be hard and tympanitic, and pain was complained of on pressure being made over the contiguous portions of the hypogastric and umbilical regions. The intestines were meteoric, and appeared figured over the whole surface. She complained of much thirst. Tongue red and contracted; pulse 120, small and feeble. For these symptoms, in many respects simulating ileus, the patient was treated with laxatives and stimulating enemata, introduced by means of the long rectum tube, with at first partial alleviation of the symptoms. A hard tumour was now detected, occupying the whole epigastric space, but the patient was shortly afterwards attacked with severe diarrhoea, from the effects of which she rapidly sunk, and died fifteen days after admission. It was not discovered, till after death, that the patient had all her lifetime evinced a morbid appetite for chewing human hair.

The following were the necroscopic appearances.

On opening the abdomen and stomach, a hairy tumour, shaped like the stomach, was extracted, which weighed twenty-one ounces,

and was found uniform in structure, except upon its greater curvature, where two parts, each an inch and a-half in diameter, exhibited a deposition of a dirty paste-like substance, and corresponded to abrasions on the mucous surface of the organ, which was everywhere found highly injected. On a section of the concretion being made, it was discovered entirely devoid of nuclei, and seemed to be composed chiefly of human hair, with here and there small remnants of thread and sponge closely agglutinated with mucous secretion, so that, when afterwards dried, it contracted to nearly one-half its original bulk. In the lower part of the ileum, near the ileo-colic valve, two smaller tumours were found, bearing the distinctive characters of the one already mentioned; and the coats of intestine lying in close approximation with the lower concretion, were found disorganised and perforated. With the exception of marks of inflammation on the outer sides of the peritonæum, and considerable distension and vascularity of the small intestines, the morbid appearances of other organs, not here specified, seemed to have no connection with the cause of death.

**11. DISEASES OF THE EYE, EAR, MOUTH, NOSE, AND THROAT.**—But a small number of patients fall to be allocated to this group, the nature of which is sufficiently indicated from its title. Out of the total 34 cases, we have 25 cures, 7 dismissed with relief, and 2 deaths. Of the deaths, one arose from *œdema glottidis* in a patient who had been submitted to the operation of tracheotomy, and the other occurred in an individual after undergoing partial excision of the lower jaw for a malignant species of *epulis*, with which he had been affected for a considerable time previous to admission.

**12. DROPSIES.**—The following classification of dropsies are inserted under a separate head, as the data from which they are collated, give no definite idea of the specific diseases on which the dropsy must depend, and of which it can at best be only recognised as a prominent symptom. Thus, the table presents a considerable number of successfully-treated cases, both of cardiac and renal dropsy, which is sufficient to show that in these cases the primary lesion was not attributable to those severe maladies, so frequently the precursors and concomitants of the dropsical effusion.

	Total.	Cured.	Relieved.	Died.
Anasarca and general dropsy,	16	10	4	2
Ascites,	20	5	8	7
Cardiac dropsy,	47	8	22	17
Ovarian dropsy,	4	0	2	2
Renal dropsy,	31	8	15	8
Hepatic dropsy,	3	1	1	1

**13. MISCELLANEOUS DISEASES AND INJURIES.**—A large number.  
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ber of diseases and injuries, that it would be impossible to classify under any physiological arrangement, are comprised under the present system. Lesions originating from external injury, as bruises, burns, and scalds; wounds, lacerated, gunshot, and incised; idiopathic gangrene; ulcers; tumours, simple and malignant; and diseases not sufficiently ascertained,—combine to represent the major part of the admissions. An analysis of the separate sources of accidents is well worthy of attention, as the results of these casualties tell seriously upon our surgical statistics. The following table, prepared from the records, gives a general view of the accident cases treated in 1848, with the mortality and the relative results in both sexes :—

Causes of the Accidents.	Total Cases.	Dismissed.			Died.		
		M.	F.	Tot.	M.	F.	Tot.
Assaults.....	16	9	7	16	...	...	...
Burns and Scalds.....	75	50	11	61	7	7	14
Coal and Lime Pit Accidents.....	35	30	...	30	5	...	5
Crushes and Bruises.....	17	15	...	15	2	...	2
Cuts & Wounds from various Instruments	10	6	4	10	...	...	...
Dog Bites.....	4	1	2	3	...	1	1
Falls on the Ground.....	50	30	19	49	...	1	1
Falls from a Height, chiefly from Scaffolds	73	43	13	56	16	1	17
Falls down Stairs.....	35	16	16	32	3	...	3
Gunshot Wounds.....	6	4	...	4	2	...	2
Kicks from Horses.....	4	3	...	3	1	...	1
Machinery Accidents.....	36	22	8	30	6	...	6
Pin Stuck in Throat.....	1	...	1	1	...	...	...
Railway Accidents.....	43	26	...	26	17	...	17
Run over by Carts, Carriages, and the like	34	26	2	28	6	...	6
Shipwreck.....	1	...	...	...	1	...	1
Spark of Iron into Eye.....	2	2	...	2	...	...	...
Weights, as Loads of Earth, &c. falling } on Patients... ..	46	39	1	40	6	...	6
Causes of Accidents not ascertained.....	4	3	...	3	1	...	1
<b>Total.....</b>	<b>492</b>	<b>325</b>	<b>84</b>	<b>409</b>	<b>73</b>	<b>10</b>	<b>83</b>

The mortality here indicated falls far short of the average amount occurring in Glasgow from the same causes, as the mortality bill of the past year cites no fewer than 272 cases referable to this head. Setting aside, however, the deaths happening immediately after the receipt of injury, and such as are depending on suicide, we have, from the tabular statement, a pretty accurate idea of the general average number of serious injuries occurring annually among the working population of Glasgow and suburbs. The most fertile sources of these returns show a close similarity to the aggregate results of former years, both in numbers and mortality, and present, as far as we know, more comprehensive data than any single institution in the country. Not a few of the cases became subjects of operation. A considerable number were utterly

hopeless from the first, and died soon after admission ; and several fell victims to the chances of delaying operation. The table on pages 260 and 261 exhibits the total operations performed for all causes during 1848.

The results of the larger amputations will be observed to exhibit the same serious amount of mortality which has of late years been attendant on this class of operations, and has rendered their performance, even in cases of urgency, a subject of the gravest consideration and doubt. Primary operations of the limbs, exclusive of the hand and foot, number 27 ; of these, 13 were cured and 14 died, thus furnishing a mortality of 51·8 per cent. Secondary amputations of a similar character were 30 in number ; of these, 10 were dismissed and 20 died, being exactly two-thirds of the cases submitted to operation. In the 27th volume of the *Medical Gazette* there is a paper by Dr Lawrie of Glasgow, detailing the results of amputations performed in the Glasgow infirmary, from its commencement in 1794 till 1838 inclusive. The series of facts there brought to light, besides showing the great mortality attending the larger amputations, went far to prove, that the causes leading to the fatal results were increasing in their operation towards the termination of the period from which the data were constructed. Since the date of Dr Lawrie's researches a period of ten years has elapsed, furnishing a large additional amount of evidence connected with the subject of amputation, and confirming in some respects the opinions formerly entertained.

My attention being forcibly directed to the subject, I felt anxious to ascertain the aggregate results of operative practice during this last decennial period, for the purposes of comparison with the previous data. With this view, all the case-books which could be obtained were examined, from 1839 to 1848 inclusive, and the results of the larger amputations carefully noted. The hospital reports for the last six years furnished the more numerous details of the primary cases occurring during the period ; and, from these returns being more explicit and comprehensive than the journals, the total number of primary cases are shown to exceed by a third the aggregate of secondary amputations. In the returns from secondary amputations it is necessary, as will be shown in the sequel, to adopt a more minute subdivision than is generally given in hospital reports. We have accordingly divided the secondary cases into two classes, whereby a complete distinction may be kept up between those amputations applicable to cases of injury, where the operation has been delayed till the failure of attempts at re-union, and those rendered necessary from disease, not unfrequently termed pathological amputations. It is scarcely necessary to add, that the term primary has been limited to those cases

## OPERATIONS PERFORMED DURING THE YEAR 1848.

Operations.	Total.	Disease or injury requiring operation.	Cause of Death.	Cured.		Relieved.		Died.	
				M.	F.	M.	F.	M.	F.
Amput. Prim. at Shoulder-Joint.....	3	Compound Fracture; Crush; Laceration.....	Phlebitis.....	1	..	..	..	..	2
.. .. of Arm .....	4	3 Comp. Fract; 1 Comp. Disloc. of Elbow-Joint...	Phlebitis .. ..	2	..	..	..	2	..
.. .. of Fore-arm .....	7	2 Comp. Fract; 3 Crush; 2 Open Wrist-Joint. ...	Phlebitis .. ..	4	2	..	..	1	..
.. .. of Hand .....	1	Smash .....	Phlebitis .. ..	..	..	..	..	1	..
.. .. of part of Hand .....	1	Laceration .....	.. ..	..	..	..	..	1	..
.. .. of Fingers.....	15	9 Comp. Fract; 5 Lacet; 1 Comp. Disloc.....	.. ..	12	3	..	..	..	..
.. .. of Thigh .....	4	2 Comp. Fract; 2 severe Laceration .....	{ Phlebitis; Pneu- monia; Shock... }	1	..	..	..	3	..
.. .. through Condyles .. .. of Femur.....	2	1 Comp. Fract; 1 Open Knee-Joint.....	Dis. of Pelvic Viscera	1	..	..	..	1	..
.. .. at Knee-Joint .....	1	Gangrene .....	Extension of Gangrene	..	..	..	..	1	..
.. .. of Leg .....	6	3 Comp. Fract; 2 Crush; 1 Avulsion ..	Shock; Pneumonia ..	2	..	..	..	4	..
.. .. at Ankle-Joint .....	2	Crush.....	Shock.....	1	..	..	..	1	..
.. .. Chopart's operation.....	2	Comp. Fract. of Metatarsus; Crush ..	Gangrene.....	1	..	..	..	1	..
.. .. of part of Foot.....	2	Laceration .....	.. ..	2	..	..	..	..	..
.. .. of Great Toe .....	1	Crush .....	.. ..	1	..	..	..	..	..
.. .. Second. of Arm .....	7	{ 3 Strumous Elbow; 2 Compound Fracture; 1 Caries; 1 Fungus Hamatodes .....	{ Phlebitis; Erysipe- las; Phthisis.... }	1	1	..	..	4	1
.. .. of Fore-arm .....	2	Crush; Phag. Ulcer.....	.. ..	1	1	..	..	..	..
.. .. of Fingers .....	10	6 Caries; 4 Crush and Comp. Fracture.....	.. ..	6	4	..	..	..	..
.. .. of Thigh.....	8	{ 3 Open Knee-Joint; 2 Comp. Fract; Necro- sis; Fungus Erysip; Gangrene .....	.. ..	..	..	..	..	7	1
.. .. of Leg .....	13	{ 4 Ulceration; 3 Comp. Fract; 2 Gangrene; } 2 Caries; Crush; Fungus .....	.. ..	..	..	..	..	6	1
.. .. of Ankle-Joint .....	4	2 Disloc. Ankle; 1 Struma; 1 Crush ..	.. ..	5	1	..	..	..	..
.. .. of part of Foot .....	3	2 Caries; 1 Commin. Fracture .....	.. ..	3	1	..	..	..	..
.. .. Chopart's operation .....	1	Ulceration .....	Diarrhœa .....	..	..	..	..	..	1
.. .. Chopart's operat, and Hay's on other Ft. }	1	Gangrene .....	Phlebitis.....	..	..	..	..	1	..
.. .. Hay's operation on both Feet..... }	1	Gangrene .....	.. ..	1	..	..	..	..	..
.. .. of Toes .....	5	4 Caries; Gangrene .....	.. ..	4	1	..	..	..	..

Excision of Elbow-Joint .....	1	Struma .....	...	...	1	...	...
... of part of Ulna .....	1	Necrosis .....	...	...	...	...	...
... of Mamma .....	1	Scirrhus .....	...	...	1	...	...
... of lower Jaw .....	1	Malignant Growth .....	...	...	...	1	...
... of Lip .....	3	Scirrhus .....	...	...	3	...	...
... of Hernia Cyst .....	1	Old Hernia .....	...	...	1	...	...
... of Necrosed Bone .....	1	Necrosis .....	...	...	...	...	...
... of Metacarpal Bone .....	1	Caries .....	...	...	1	...	...
... of Metatarsal Bone .....	1	Caries .....	...	...	1	...	...
... of Os Calcis .....	1	Caries .....	...	...	1	...	...
Extirpation of Testicle .....	2	Malignant Growth .....	...	...	2	...	...
... Fungus of Scalp .....	1	...	...	...	...	1	...
... of Encysted Tumours .....	2	...	...	...	...	...	...
... Malignant Tumours .....	3	...	...	...	2	...	...
... Scrofulous Tumours .....	3	...	...	...	2	...	...
... Cartilaginous Tumours .....	2	...	...	...	2	...	...
Section Subcutaneous, Tendons ..	9	Talipes .....	...	...	6	3	...
... Cicatrices .....	4	1 Burn ; 2 Cancerum Oris ; 1 Stricture of Rectum	...	...	2	2	...
... of Anal Fistulæ .....	12	...	...	...	9	1	...
... Hæmip .....	3	...	...	...	2	1	...
Reduction of Hernia by Taxis .....	1	Oblique Inguinal .....	...	...	1	...	...
... of Hernia by operation .....	2	Oblique Inguinal Strangulated ..	...	...	...	...	...
... of Humerus .....	7	6 Downwards ; 1 Forwards, Dislocation.	...	...	7	...	...
... of Elbow-Joint .....	1	Backwards Dislocation .....	...	...	1	...	...
... of Ulna .....	1	Outwards Dislocation .....	...	...	1	...	...
... of Thumb .....	2	Backwards Dislocation .....	...	...	...	...	...
... of Femur .....	1	Upwards Dislocation .....	...	...	2	...	...
Laryngotomy .....	1	Edema Glottidis ; Tumour over Trachea ..	...	...	1	...	...
Ligature of Nævus .....	3	...	...	...	1	...	...
Lithotomy .....	7	Calculus in Vesica .....	...	...	7	...	...
Puncture of Bladder above pubis ..	1	Congenital Stricture .....	...	...	...	...	...
Operation for Hydrocele .....	12	7 Tapped ; 5 Injected .....	...	...	9	...	...
Suture .....	1	Vesico-Vaginal Fistula .....	...	...	...	...	...
Circumcision of Prepuce .....	2	Primary Syphilis ; Congenital Phymosis ..	...	...	2	...	...
Talacotian operation .....	1	Burn of Neck .....	...	...	1	...	...
Trepphine .....	3	Injury of Skull .....	...	...	...	3	...
Urethrotomy .....	2	Calculus in Urethra .....	...	...	2	...	...
Total .....	207	...	...	...	118	28	7 4 43 7

only where amputation has been performed in the acute stage immediately following the receipt of injury.

The following table presents an abstract of these inquiries, and includes all the larger amputations with the exception of the foot and hand :—

Nature of Amputation.	Total.	Fore-arm.		Arm.		Leg.		Thigh.		Mortality per cent.
	Cured & died.	C.	D.	C.	D.	C.	D.	C.	D.	
Primary, .....	169	31	4	34	15	31	22	11	21	36·6
Secondary from injury,...	56	3	..	7	9	6	13	3	15	66
Secondary from disease,	59	3	2	4	5	14	9	15	7	38·9
Total,	284	37	6	45	29	51	44	29	43	42·9

Of these, 251 were males and 33 were females; of the former, 138 were cured and 113 died; and of the latter, 24 were cured and 9 died. It follows, therefore, that of the total operations, 162, or 57 per cent., were successfully treated, and that 122, or nearly 43 per cent. of the whole number of cases, died, thus the table presents a surplus mortality of  $6\frac{2}{3}$  per cent. over the ascertained results of the practice of the hospital during the first quarter century of its existence. But the most remarkable feature in the present returns is the estimated success of the primary over the secondary amputations. Being somewhat scrupulous in admitting this inference, I was induced to re-examine the various sources of information, to try to discover any inaccuracies which might have occurred on a first perusal. This revisal has tended rather to strengthen than to question the results of the previous inquiry. Secondary amputations from injury furnish 56 cases, 19 of which were successful, and 37 terminated fatally. The mortality from the present class of operations was formerly estimated about 56·6 per cent., but during the period now under consideration it appears to have increased to 66, or nearly two-thirds of the cases submitted to operation. Here, then, is abundant evidence to show the dangers attending the performance of secondary amputation from injury; and if we add to the already large mortality list the deaths occurring in cases where no secondary operation could be performed, in consequence of the supervention of constitutional and other complications, the risks of the practice will appear still more seriously hazardous. The results of the primary cases are more satisfactory;—of 169 cases, there were 107 dismissed and 62 deaths, which is equivalent to a mortality of 36·6 per cent. The deduction exhibits a favourable contrast when compared with Dr Lawrie's statistics, in which the recoveries and deaths are found to be nearly equal; or to the more recently-published researches of Malgaigne,

extending over a like period of ten years, where the mortality in the Parisian hospitals is shown to exhaust nearly two-thirds of the whole admissions. On the other hand, the history of pathological amputations exhibits a diminishing ratio of success;—of 59 cases reported, 36 were cured and 23 died, being equivalent to a mortality of 38 per cent. The figures show a surplus mortality of 16 per cent. over the previously-published data, and exceed, by  $2\frac{1}{2}$  per cent., the deaths from the present statistics of primary amputations. It will be observed, however, that in the more serious amputations of the leg and thigh, the results have been much more successful than in similar operations performed during the primary stage following on injury; and if we exclude altogether from the table cases of amputation of the fore-arm (which is generally a successful operation), the mortality of the primary cases, instead of averaging 36, will rise as high as 43 per cent., while the results of pathological amputations will not be sensibly affected. Notwithstanding these redeeming features, it must necessarily follow, that while the results of primary amputations have become more favourable during the last ten years, the success of pathological amputations appears to have taken a retrograde movement, and that secondary operations performed for traumatic lesions have become fatal in the extreme. Judging from the returns at different periods, we must also infer that the causes, leading to the unfavourable results in secondary amputations for disease, are increasing in their operation along with the continued existence of the hospital itself. Thus, from 1794 to 1810 inclusive, Dr Lawrie specifies 30 cases with but 1 death occurring; whereas in the last 30 happening before 1839, there were 8 deaths to 22 recoveries. Since that period the mortality has increased even in a greater ratio, until it has reduced our estimate to the present large percentage. How then, it will be asked, are these serious discrepancies to be explained?

Few subjects connected with surgical pathology have of late years engaged so much attention as the statistics of amputations. The researches of Philips, Lawrie, Malgaigne, and Simpson, have done much to expose the dangers of operations, more especially in hospital practice, besides showing the influence of external causes on the question at issue. At the same time, we must be alive to the fact, that the very knowledge of these dangers has a reflex effect upon the mortality, by deterring surgeons from having recourse to the knife until every other therapeutic agent fails, and then only as the last alternative for prolonging existence. Statistics from all sources go to prove the less frequency of operative interference in disease now than formerly. The same limb which would have been unceremoniously lopped off some thirty years ago by our predecessors, is, in our own day, allowed its chances of re-

union, and the benefit of those expedients which modern surgery has found serviceable, both in the mechanical and constitutional treatment of disease. From the introduction of these measures, pathological amputations are being more and more confined to the few, many of whom have their strength previously undermined by a constitutional taint, or exhausted by some continuous discharge, rendering the chances of operative success extremely doubtful. Again, the dangers attending the secondary class of amputations for injury are rendered in a greater degree imminent from the previous shock and often complicated accident sustained by the patient, besides the severe ordeal he has to pass through from a residence in a surgical hospital. We question much if the boundary line existing between the two extremes in practice above referred to has ever been accurately determined. Mere statistics from collated experience on the results of amputations afford few satisfactory elements for the solution of this most important problem.

To arrive at any general inference (for accuracy in a point beset with so many difficulties is scarcely attainable), we would require to possess as comprehensive data in connection with those cases in which operative interference was deemed inadvisable, as we at present possess of that class which have been subjected to its performance. I regret that, not having my attention directed to this subject when examining the records for the last decennial period, I am unable at present to furnish data bearing upon the question. By a little diligence, however, a vast amount of practical information could be readily obtained, going far to determine a point of practice which will continue to maintain an importance commensurate with the increasing demands for labour in a commercial city.

While our statistics go to favour the adoption of early amputation, we cannot be regardless of the fact, that the mortality in this, as in most hospitals, is seriously influenced by local causes, exclusive of those already mentioned. In the Glasgow hospital, the most frequent complications are such as depend on the prevalence of epidemic gangrene and erysipelas, while the most formidable, as they are almost uniformly fatal, owe their origin to the insidious occurrence of purulent depositions in the vital organs, accompanied with the supervention of typhoid symptoms, arising from inflammation of the veins of the amputated member. I believe that I am rather below than above the mark, when I state that, of the total deaths occurring after amputation, one-third may be fairly ascribed to the cause last mentioned. All cases are alike subject to its influence. But it shows a greater disposition to ensue after amputations for injury, and especially secondary operations of that character, than it does pathological amputations. In the list of casualties ranked among the necessary evils which attend on hospital practice, there are none which weigh so heavily with the surgeon in

his attempts to save life and limb as the insidious complication now under consideration. For, while other secondary affections are anticipated, and may to a certain extent be remedied, the most assiduous care and well-founded hopes of success, indicated by the improved condition of the patient, are too frequently frustrated by the sudden occurrence of the fatal rigor, and the train of symptoms so peculiarly characteristic of hospital phlebitis.

In the anxiety to discover the causes concerned in the production of this disease, various opinions have always been entertained by different observers; and among the more probable suggestions we may cite overcrowding, want of ventilation and cleanliness, and neglect of other hygienic precautions. None of the causes here indicated are sufficient to explain the super-vention of secondary diseases in the Glasgow hospital. They make their appearance in winter as well as in summer, in foggy conditions of the atmosphere as well as in dry, when the wards are in one-half vacated, as in seasons when every bed is occupied; and we are unwilling to admit that essential cleanliness in all departments was not rigidly observed. Such appears also to be the general inference, from observations deduced from collateral sources; for there are, few if any of our hospitals, that can boast an entire immunity from these local complications. In the lately published report for 1848 of the Perth hospital, it is stated "that the evil" (which in this case was epidemic erysipelas) "had increased so greatly, that it became a matter of grave doubt whether a surgical operation, however simple, ought to be performed within the walls." The managers of that institution have, in consequence, authorised steps to be taken for improving the ventilation, painting and cleaning the wards, procuring new bedding for the patients, and otherwise renovating the interior of the hospital. How far the adoption of such measures, which are also in contemplation by the managers of the Glasgow Infirmary, will prove useful in checking the evil, we are not prepared to say. The experiment itself will no doubt afford full scope for future analysis.

Another plan recommended for checking these affections, consists in the removal of the patients to a tenement previously unoccupied, where no danger is to be apprehended from infection latent either in the atmosphere, or in the stone and mortar of the building.

With the view of testing the alleged benefits of the system of temporary segregation, a trial was made in the Glasgow infirmary during the summer months of last year, and the greater number of the capital operation cases were accommodated for a time in the wooden shed, previously constructed for fever patients. Fourteen capital operations were performed under these circumstances, of which 8 were successful and 6 died. Of the 14, 8 were pri-



mary and 6 secondary; 3 of the primary cases died, and 5 recovered. Among the recoveries are included a case of amputation through the knee-joint, and the only successful primary amputation of the thigh dismissed during the year. Of the secondary amputations, 3 were the result of injury, all of which terminated fatally; the remaining 3 were the consequence of disease, and were dismissed in a fair way of recovery. We feel it would be premature to deduce any practical inference from the limited nature of the preceding returns; at the same time it must be allowed, that, from the very serious nature of the cases, the results are sufficiently conclusive to warrant at least a further trial of the experiment. Another subject, to which we will be excused for adverting, is the relative success attending amputations in different hospitals.

The information we possess on this head is exceedingly limited; being almost entirely confined to a paper by Dr Simpson in the April number for 1848, of the *Monthly Journal of Medical Science*. It would appear, from Dr Simpson's inquiries, which include the results of 618 amputations, from the experience of 30 British hospitals, that the mortality over all did not exceed 29 per cent. of the cases,—a mortality much below the standard of other authorities. These results were, however, collated in a great measure from the records of provincial hospitals, many of which have but recently been instituted; and it is only fair to surmise that the success of operations in such establishments will be greater than that of larger and older institutions. The physical conditions of the patients in either case must also have a tendency to influence the results, as it can scarcely be expected that the chances of success attending the convalescence of that class previously accustomed to a country life, will stand comparison with the poor, half-starved, ill-conditioned wretches who form a large proportion of our operation cases. Whether the introduction of anæsthetic agents into the operative practice of the Glasgow hospital has had any influence on the success or mortality is still doubtful, as the data we possess regarding this point are not sufficiently comprehensive for analysis. Dr Simpson's recorded results of 302 amputations, performed during the anæsthetic state, certainly favour the supposed therapeutic efficacy of the agent, by reducing the mortality to so low a standard as 23 per cent. It will, however, be borne in mind, in drawing any general conclusion from these returns, that, at the period when Dr Simpson's statistics were collated, the administration of the agent was far from being so universally had recourse to as it now is. At that time, many surgeons, impressed with the pardonable timidity which will ever accompany the introduction of any new and powerful agent into the practice of medicine, relinquished its application in cases of extreme exhaus-

tion, which were least likely to arrive at a favourable issue under either circumstances. Such we know was the practice in the Glasgow hospital, and we observe similar opinions to have been entertained by Velpeau (Lancet) in the course of his clinical lectures on the cases occurring after the streets combats of June 1848. It is gratifying to find that these opinions have been gradually abandoned, as no longer tenable, and that increased experience has tended only to confirm the harmless nature of the agent when administered with becoming care.

# FEVER DEPARTMENT.

In a previous page an abstract is given of the total fever cases treated in the course of the past year, from which it may be shown that the total admissions have so far diminished, as to constitute little more than one-fourth part of the number treated in 1847, while the mortality, from the same cause, has continued nearly in an equal ratio with the previous returns, the general average yielding about 1 death in every 5 males, and 1 in 8 among females. The majority of the returns were furnished during the spring months, when fever was still epidemic, although rapidly declining in intensity, and the remaining portion must be considered as examples solely of the endemic disease, as little alteration took place in the general returns after the month of May, at which period the disease exchanged its epidemic for its endemic character. The following table presents an enumeration of the various types of fever treated in the fever wards in 1848, with the numbers cured and dead. A variety of other affections, also treated in these wards, are purposely excluded from the present enumeration, since they belong, in a more particular manner, to the medical and surgical department:—

Diseases.	Total Cases.	Cured.	Died.	Mortality per cent.
Typhus, ...	807	654	153	18·95
Do. with complication, ...	173	133	40	23·12
Continued and Relapsing Fever, ...	419	419	...	...
Do. with complication, ...	94	70	24	25·53
Small Pox, ...	48	35	13	27·0
Scarlet Fever, ...	18	17	1	5·5
Febricula, ...	15	15	...	...
Dothinenteric Fever, ...	7	6	1	14·28
Influenza, ...	6	6	...	...
Intermittent Fever, ..	1	1	...	...
Total,	1588	1356	232	14·6

When the late epidemic of fever was at its maximum in Glasgow, it presented an almost equal division of two distinct types, namely, typhus and relapsing fever, each bearing well-marked

characters, and differing essentially in their relative mortality. As the disease declined, the ratio of the two sets of cases became considerably altered. Relapsing cases decreased rapidly in number during the last months of 1847 and early part of 1848, while, at the commencement of our present statistical year, typhus cases nearly doubled the amount of the other, and continued to retain the duplicate ratio during the whole course of the year. The typhus cases presented little worthy of note, apart from what are well known to be the leading features of the malady in this part of the island. The reported number affected amounted to 980, of which, 604 were males, and 376 were females. Of the former, 134 died, and of the latter, 59 died; the mortality over all averaging 19·69 per cent., or about 1 in 5. The co-existence of internal complications with the sequelæ of the disease itself have had less influence on the gross mortality than we might at first be disposed to anticipate,—a difference of four per cent. of increase being the amount attributable to cases of mixed typhus over the simpler and uncomplicated forms of the malady.

Among the most frequent complications are recorded cases of erysipelas and inflammatory affections of the respiratory organs, especially of the lining membrane of the bronchi. Of this last, 61 were cases reported, with but 4 deaths, a very small mortality when compared with previous returns, and averaging much less than the recorded results from pure typhus. The numbers, moreover, are not intended to represent the total bronchitic complications, as the extreme frequency of their occurrence in the course of typhus forbids us specifying any but those, in which the nature of the complication was of such severity as to admit of separate registration.

Erysipelatous complications being extremely prevalent in our fever wards during the early part of 1848, have added more to the mortality in mixed typhus than any other class of complications. Of these, 43 cases were reported, with 13 deaths. Of 9 cases reported as complicated with pneumonia, there were 3 deaths.

There were also 5 cases attacked with cholera during the continuance of, or in the course of, convalescence from typhus, in which condition they were removed to the cholera hospital. The remaining complications consisted partly of diseases pre-existing among the patients,—as struma, phthisis, and syphilis,—and partly, also, of abscesses and morbid discharges, the sequelæ of the fever itself, in which cases, more inconvenience was to be apprehended from retarded convalescence than from the fatality attending their progress.

The disposition of the other forms of continued fever to pass almost imperceptibly from one grade to another, has been well

illustrated by the changes which the relapsing or famine fever of 1847 has undergone in the course of the past year. Towards the termination of 1847 and the commencement of 1848, the relapse, usually considered the pathognomonic feature of the malady, was frequently absent, and the primary attack became prolonged to an indefinite extent, but the cases still retained the other symptoms characteristic of the disease at this particular period. As the year advanced, and the numbers diminished, the relapses became less frequent, until they began to form the exception rather than the rule; and the disease ultimately assumed a mild form of synchus, the characters of which it has continued to retain to the present time.

In consequence of the data afforded me for registration not being sufficiently explicit to denote the several varieties of these affections, I have been compelled to classify the whole under one head in the manner adopted in the table. Nor, for several reasons, could much dependence be placed on a more minute classification. At the period under consideration, the disease was evidently undergoing a transition state, and seemed to occupy, for a time at least, a position intermediate between the two great links of the Cullenian division, *synocha* and *synchus*. Many cases that, on admission, presented the customary characters of the relapsing epidemic, began ultimately to assume a more continued type, so that their individual registration might, with sufficient propriety, have been affixed to either head.

Apart altogether from this close analogy in the symptomatic manifestations of a later date, there were other points of similarity existing, tending much to favour the opinion of the identity, or at least of the closely-approximating nature, of these two forms of continued fever. Among the most prominent, I may mention the duration of the disease, the mildness of the symptoms, and consequent limited mortality, the proneness to inflammatory complications of the respiratory organs, and the capability of the one form in producing the other, being confined to separate instances, and never occurring in the same individual. In these phenomena, as well as in other details, principally of an etiological character, a closer resemblance is observable than we are in the habit of meeting with among the typical characters of febrile diseases; a circumstance which goes far to establish the position, that the famine fever of 1847, and the mild form of continued fever which succeeded it, were merely modifications of one and the same disease. In the previous instance, all the symptoms save one important periodic distinction accorded well with the established definition of *synocha* by Cullen. The relapse, which was almost an invariable accompaniment in the course of the epidemic, certainly favoured the supposition entertained by many observers,

that the disease was one *sui generis*, and differing essentially from other forms of continued fever, while others deemed its occurrence sufficient to exclude it entirely from the pale of the Cullenian division. I am sanguine enough to believe, however, that as the epidemic declined, and the relapse, from its infrequency, began to lose its pathognomonic importance, the conflicting opinions regarding the identity or non-identity of the malady with continued fever have been in a great measure abandoned and been resolved into the admission of the former supposition.

To those who attach much importance to varieties in certain symptoms, and thereby encourage multiplicity in the nosological arrangement of fevers, each succeeding epidemic affords ample room for speculation and disputed theories on a subject already too much hampered with the same. In this, as well as in most other diseases of the zymotic class, increased experience shows the necessity of assigning due weight to that mysterious series of periodic revolutions, which since the days of Sydenham have received the somewhat equivocal appellation of the "epidemic constitution of the time," and which doubtless tend to modify the characters of such recurrent epidemic attack.

There is perhaps no point in the history of the late fever more clearly established than the fact of its having been primarily imported from Ireland. Observations made during the origin and subsequent progress of the malady demonstrate its connection with the great amount of destitution which prevailed among the lower orders of Irish at the period, chiefly in consequence of the deprivation of one important article of food on which these classes mainly depended for subsistence. The existence of the fever in Ireland prior to its appearance in this country, and its almost simultaneous outbreak in the maritime cities of the west coast, after an unusual amount of pauper immigration from Ireland, are facts strongly illustrative of the extraneous origin of the malady, and, in short, follow each other on the general principles of cause and effect. But it appears from our returns, that of the two dominant types of fever, the relapsing form had much stronger claims to a foreign origin than typhus, if indeed the spread of the latter could be at all ascribed to this cause. Typhus is constantly present in Glasgow, and although apparently subject to certain laws of periodic exacerbation, it is not the less liable to the action of unforeseen circumstances tending materially to its diffusion. In the course of the past epidemic, the number of typhus cases kept in abeyance for a considerable period posterior to the general diffusion of the relapsing fever; and, as a general rule, the former disease was much more obnoxious to the Scotch resident population than the Irish, while the latter or true famine fever was mainly confined to the low Irish settlers;—whether we take it in

its outbreak, subsequent progress, or later decline, we distinguish the same causes in operation, as if seemingly engrafted on the Irish constitution. The following statement gives the relative numbers of Scotch and Irish affected with the separate types of fever during 1848 :—

Scotch.		Irish.	
Typhus fever, . . .	617	Typhus fever, . . .	363
Synochus and synocha, .	285	Synochus and synocha, .	228
Total, 902		Total, 591	

As compared with the previous year, these figures show a sensible decrease in the number of Irish below the Scotch admissions, and likewise exhibit the greatly-diminished number of admissions of both sets of cases, more especially of the milder instances of the disease, which amount to little more than one-half of the typhus cases. Hence it follows, that, as the epidemic declined, it exhausted its energies among the resident population, and was diffused as widely among the Scotch as among the Irish residents, but that, under certain restrictions, both types of fever continued to maintain the national characters which marked their advent. The disproportion between the two sets of cases, as observable in the table, was steadily kept up during the past year, and an examination of the present year's returns would indicate a still greater disparity in the numbers of the synochoid class, since maculated typhus has lately nearly absorbed the whole fever in hospital. It is of importance to bear in mind these changes, when judging of the average mortality from fever in different hospitals and at different periods.

The discrepancies so apt to occur in making a comparative estimate of the total returns of one year with another, may thus be readily accounted for, and due importance be attached to the occurrence either of an unusually severe and fatal form of the disease, or one attended with few serious consequences. Of the last-named character was the continued fever of the past year, which we previously observed, approximated closely in many respects to the relapsing fever which preceded it. In its uncomplicated form, saving the co-existence of advanced age, purposely classed under the mixed head, the cases present no mortality. The number of mixed cases amounted in all to 94, of which 24 terminated fatally, a somewhat larger proportion of mortality than that attending complicated typhus.

Of the above 94 cases, 34 were complicated with bronchitis, out of which 4 died; 5 cases were attended with inflammation of the lungs, of which 3 died. Peritonitis, almost invariably a fatal complication in the relapsing fever of 1847, co-existed in 5 cases, all of which terminated fatally. Abortion occurred in 4 cases,

of which 2 died. Diarrhoea and dysentery, 6 cases, with 2 deaths. Erysipelas, 6 cases, and 1 death. There are 7 cases reported of patients who, on admission, were affected with synchoid fever, but afterwards underwent an attack of some other contagious disease. Of these, 4 had typhus, who all recovered; 2 had modified small-pox; and 1 had a severe attack of scarlatina, the last proving the only fatal complication of this group. The remaining 27 complications were attended with 6 deaths, but, from their miscellaneous character the details are uninteresting, and we forbear making special allusion to each.

#### CHOLERA DEPARTMENT.

The re-appearance of cholera in this country towards the latter part of the past year, and its anticipated visitation of Glasgow, compelled the authorities to put in full exercise those powers with which the legislature has of late years endowed them, for the mitigation of famine and disease. In the course of the late epidemic they received the joint co-operative assistance of the managers of the Infirmary, and among other arrangements made previous to the outbreak of cholera in the city, it was agreed, that that portion of the Infirmary buildings, in common use as a fever hospital, should be set apart solely for the accommodation of cholera cases. With this view, the resident fever cases, which at the period were considerably reduced in number, were removed to another part of the establishment, and measures were taken for the effectual isolation of the three separate classes of patients, so as to afford for each ample accommodation, and at the same time tend to allay the fears of the most fastidious contagionist.

The first case of cholera occurred in Glasgow on the 5th of November, in a man who had returned from the neighbourhood of Edinburgh on the previous evening, where he had been engaged in the interment of a deceased relative who had fallen a victim to the malady. The second occurred on the 8th, and the third on the 12th of the same month, after this time several sporadic cases appeared in various parts of the town; but, on the whole, the disease proceeded during the remaining part of the month with that tardy progress which so frequently marks its insidious outbreak, misleading the uninitiated with the anticipation of a partial visitation.

The first cases occurring in the practice of the Infirmary were admitted on the 27th November, at which time the wards may be said to have been nominally opened for their reception. From this date till the close of the year, when the epidemic had evidently reached its acme, the admissions increased weekly in number, and were attended with a larger proportion of mortality than

the cases occurring subsequently. The first week of January witnessed a slight diminution in the number of the cases; but the numbers remained nearly at an equilibrium till the end of the third week. After this they gradually diminished, comparatively few cases being admitted in February, and the hospital was finally closed on the 10th of March, no admissions having taken place for some days previous to closure. Although our total cases, for obvious reasons, were exceedingly limited, I believe that the foregoing illustration of the progress of the disease will afford a faithful index of the manner in which it spread more generally over the city. The following statement gives the total numbers treated in the Infirmary during the period specified, as also the results:—

Total.	Cured.		Died.	
	Male.	Female.	Male.	Female.
190	83	44	64	44

Reducing the above numbers to a per-centage mortality, that of males will be found to amount to 62·74, that of females to 50·, while the mean fatality is estimated at 56·84. The generality of the cases were of the very worst description, exhibiting the most malignant features of the malady, and hence the attendant high rate of mortality. It is nevertheless fair to add, that several of the recoveries classified in the above category could not be ranked as cases of Asiatic cholera. Among the number, 2 were reported as having suffered merely from the effects of apprehension, 3 from diarrhœa; and, although the remainder were returned as instances of cholera, I have sufficient grounds for affirming that several of these were merely examples of that species of epidemic diarrhœa peculiar to the period, and so universally prevalent, that few escaped an attack. Such, however, must be reckoned as exceptions to the general run of the cases, as they form but a small proportion of the total admissions; and it is perfectly obvious; in estimating the results of treatment in cholera, that, in proportion to the numbers thus affected, so will the mortality be influenced. Perhaps this observation may in part account for the alleged superiority of private over hospital practice. In the one case, the physician sees his patient at the commencement, and, by the timely use of opiates and astringents, frequently cuts short the disease; while, in the other, he is forced more exclusively to grapple with the malignant train of symptoms too often the harbingers of speedy dissolution, and which have hitherto baffled all the resources of medical skill. It was in this last condition, when the premonitory stages had been superseded by the true choleraic, that the great bulk of the cases were received into hospital.

The previous habits of those attacked have doubtless also a considerable effect in raising the standard mortality. With a view of ascertaining how far these conditions influenced the liability to



the disease and its consequences, the following table has been constructed, embracing the whole of the cholera admissions, and the results of treatment in each separate class of patients. The information which it affords is moreover more to be relied on, as the results were obtained by an individual visiting the houses of the sick, and securing, by personal observation and strict inquiries of relatives and friends, what it would have been impossible to exact from the patients themselves.

	Total.	Cured.		Died.	
	M. & F.	M.	F.	M.	F.
Temperate, and in moderate circumstances,	60	19	21	12	8
Temperate, but in destitution,	75	16	14	24	21
Destitute and dissipated,	40	1	6	22	11
In moderate station, but dissipated,	15	2	3	6	4
Total,	190	38	44	64	44

In all epidemics attacking an adult population, we invariably trace some connection between the operation of the poisonous agent and the physical organism ; and it is at least satisfactory to know, that by the proper regulation of the laws which govern our being, we are rendered, to a certain extent, free from the noxious influence. By the infraction of these laws, on the other hand, whether in a moral or a physical sense, the balance between health and the operation of external agents is subverted ; the constitution is thrown open to the invasion of multitudinous evils ; and man is compelled to pay compound interest for the debt which he originally owes to nature. Such at least is the result of observations deduced from everyday experience, and as its application holds good in those extensive visitations of fever to which this country is ever and anon subjected, the like principle will be admitted in the case of epidemic cholera. But the facts speak for themselves. The numbers, it is true, are limited, nor do we possess any criterion whereby we can judge of the average numbers of each of the above-mentioned classes in a given mass of population ; but the results are not on this account less valid and trustworthy. The destitute, the infirm, and the intemperate are beyond all proportion the most frequent victims of the malady. It would be urging the matter too far to affirm, that the well-lived, the robust, and the vigorous were in no danger of becoming affected. Unhappily we have too many instances to the contrary ; but their cases are chiefly sporadic ; they form no nucleus for the further dissemination of the fatal poison ; they appear sacrifices to inordinate susceptibility, and, in virtue of their previous habits, death is dealt out with a more sparing hand. The pestilence, indeed, knows its victim. Its mission to the healthy is less frequent, worse defined, and may often be stayed in its onward course ; but there are few points better established than that it descends with unsparing malignity on

those who, in defiance of its appointments, continue in the indulgence of their vicious propensities. Those causes operate with the same force among the inhabitants of the remote deltas of the Ganges as amid the busy haunts of our native cities. The following table represents the ages of the patients attacked, along with the results at each period :—

Ages.	Total.	Cured.		Died.	
	M. & F.	M.	F.	M.	F.
From 1 to 10 years,	16	4	4	4	4
" 10 to 15 "	16	4	6	4	2
" 15 to 20 "	25	7	10	6	2
" 20 to 25 "	22	3	5	8	6
" 25 to 30 "	40	11	6	15	18
" 30 to 40 "	37	4	6	14	13
" 40 to 50 "	22	4	5	9	4
" 50 to 60 "	3	0	0	1	2
" 60 upwards,	9	1	2	3	3
Total,	190	38	44	64	44

We notice in some respects a difference between the results of the foregoing table and the ages of the patients proved to be most prone to attacks when cholera was epidemic in this country in 1832. At that time, as far as we can learn, the aged suffered in much greater proportion than the young or middle-aged; and children were by no means so frequently the victims of the disease as they have been during the present epidemic. This fact was corroborated by the statistical records of the hospital in Clyde Street, the building set aside by the city parochial authorities for the accommodation of cholera cases in that part of the town, and which received nearly 1000 patients during the eighteen weeks the epidemic prevailed.

From the fatal rapidity of the symptoms, after the disease had fairly established itself, the average residence of the cases was necessarily very limited. The stay of the fatal cases averaged over all about 56 hours, varying in extent according to the manner in which death occurred. Thus, out of the 108 deaths, 60 happened before the residence of the patients had exceeded 48 hours, while the remaining 48 occurred at varying intervals after that period. A considerable proportion of the last mentioned class were instances of the typhoid reaction or secondary fever of cholera, a phase which the malady frequently presented, especially in those patients who were fortunate enough to pass scathless through an extreme state of collapse.

In the successful cases, the convalescence, although not so rapid as we were disposed to anticipate from the accounts of the former epidemic, was still, on the whole, short and satisfactory, being seldom protracted beyond 14 days, and averaging over all about 10 days. With but one or two exceptions, the patients suffered little from relapses or sequelæ of any kind. One old woman suf-

ferred much from extensive abscesses in her right shoulder and arm, but was ultimately dismissed well after a protracted residence of 66 days. Another female, aged 36, may be said to have been the only case in which the affection of the gastro-intestinal canal was followed subsequently by true organic lesion. This patient remained in the hospital with symptoms of chronic dysentery until the wards were closed, after which she was removed to the medical wards, where she gradually sunk and died, 71 days from the period of admission. The principal inconvenience in the course of convalescence was, in fact, chiefly attributable to the profuse salivation that the patients underwent from the administration of large quantities of calomel in the early stages.\* Notwithstanding the havoc the disease made in many of the public institutions, it affords agreeable reflection that so few cases should have occurred within the other departments of the hospital, while cholera was at the same time extremely prevalent in the neighbourhood of the house.

In the medical and surgical department, only one patient was seized, a young woman, who had undergone amputation of the arm, and who died after a 48 hours' illness. In the fever wards, 5 cases occurred in all, 4 supervening upon fever, and, in the remaining case, the choleraic symptoms appeared while the typhus exanthem was still distinctly visible, exhibiting the rare pathological phenomenon of two essentially distinct epidemics co-existing in the same individual. Of the 5, 3 died and 2 recovered, one poor girl tenaciously, notwithstanding the successive attacks of severe typhus, the extreme collapse of cholera, and an aggravated form of consecutive fever.

Instances of the disease among the officials were entirely confined to those in connection with the cholera wards. Four cases were reported under these circumstances, 3 of which, comprising a laundress, a nurse, and a bathman, recovered, while the remaining case, the only one of the number presenting the true algide symptoms, died three days after the first appearance of the symptoms. The subject of this last was a half-famished individual, who had been engaged a short time previously to act in the capacity of night porter, and who, in the course of his illness, pertinaciously ascribed the primary disorder to the fact of his having respired, for a few moments, the close and polluted atmosphere of a room while removing a cholera patient to hospital. I believe that we effected much good, both in facilitating the recoveries among the patients, and, to a certain extent, protecting the ward officials, by adopting a few precautionary measures with respect to the allocation of the

\* For an account of the treatment pursued in these cases, see an interesting paper in one of the numbers of the Medical Gazette for August last, by Dr M'Gregor, physician to the hospital.

patients and the condition of the wards. At the first outbreak, one double ward sufficed to accommodate all the cases without any tendency to overcrowding; but as the numbers increased, towards the end of December, we found it necessary to open two additional wards, setting them apart chiefly for convalescents and cases of secondary fever. In the receiving ward we retained 3, and sometimes 4 nurses, the number of patients never exceeding 13, and averaging generally 8 or 9, while, in the others, 2 nurses to each were considered a sufficient complement. The temperature of the receiving ward was kept usually as high as 70° or 75° of Fah., so as to facilitate the measures employed for the maintenance of the animal heat; and due precautions were used, by the occasional temporary vacation of the ward, for the better prevention of the evils that might have ensued from over-concentration. The advantages accruing from the adoption of these measures, especially of the last-named, were exceedingly obvious; and should the epidemic again revisit us, which, in the course of events, we cannot deem improbable, there is no point on which I would feel it more my duty to insist, than that of vacating, from time to time, the accommodation afforded for the affected, and by the liberal use of what disinfecting agents we possess, endeavour, as far as possible, to counteract the concentrated influence of the poison.

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ART. II.—*Some Account of the Fever which recently prevailed in the Garrison of St Ann, Barbados.* By JOHN DAVY, M. D., F. R. S., London and Edinburgh, Inspector-General of Army Hospitals, &c.

THE garrison of St Ann is situated on the south-west coast of Barbados, about a mile and a quarter from the principal town of the island Bridgetown, less than a quarter of a mile on an average from the sea-shore, and on ground very little elevated above the level of the sea, probably, where most so, not exceeding 30 feet, and where least almost on a level with the sea-beach. Within its area are comprised four separate barracks capable of accommodating about 1500 troops, some detached quarters for officers, two hospital buildings, and an extensive parade ground of many acres, commonly called the Savannah, on or on the skirts of which the several buildings mentioned have been erected.

In December 1847, when the fever, the subject of this communication, first made its appearance at St Ann, the troops in garrison there were the 7th Royal Fusiliers, about 520 strong,

occupying the brick barracks, at the head of the savannah ; the head quarters of the 88th regiment, consisting of four companies about 378 strong, occupying the stone and iron barracks at the opposite side of the parade ground, distant from the former about 1600 feet, and close to the public road from Bridgetown ; the whole way an almost uninterrupted street ; a detachment of the Royal Artillery, about 99 strong, occupying the artillery barrack, about 200 feet distant from the stone barrack in a northerly direction ; and a detachment of black troops and of military labourers, also Africans, together about 249, occupying the old dock-yard barrack, only a few yards from the sea-beach.

The time of the commencement of the fever was towards the middle of December. The first well-marked case was one which proved fatal in a private of the 88th regiment, having been admitted into hospital on the 14th of that month.

From that date to the 20th of March following, this regiment lost 28 men, 3 officers, and 4 women, victims of the disease. The royal artillery lost 2 men. The 7th Royal Fusiliers sustained no loss, nor did the African troops.

On or about the 13th of January the iron and stone barracks were vacated by the 88th, who were encamped on the savannah in the rear of the brick barracks, where they remained till the period arrived for the yearly change of stations, viz. the latter end of March.

The removal of this corps was preceded by the disembarkation of the 66th and 72d regiments from the Mediterranean in February and March, and was speedily followed by the departure of the 7th Royal Fusiliers for Nova Scotia, and of a detachment of the Royal Artillery for England, relieved by one from thence, recently arrived.

The 66th regiment, on landing, was encamped on the savannah, where it remained under canvass till the first week of April, when it occupied the stone and iron barracks, previously painted and white-washed, after having been vacated eight weeks. The 72d regiment, on landing, were in part quartered in the brick barrack, and in part encamped in the rear of those barracks, till by the departure of the 7th the remainder could be similarly accommodated. The detachment of the Royal Artillery was also at first placed under canvass ; but shortly after the embarkation of the detachment relieved they occupied their own barracks.

A hope at this time was entertained that the fever which had proved so destructive in the 88th had ceased, and that the newly arrived troops would escape its ravages. The expectation was founded in part on experience, showing that fever of this kind is commonly of very limited duration, seldom exceeding the term of two or three months, and partly on the cir-

cumstance of its apparent cessation in the 88th, prior to their removal. This hope, however, proved to be false. Doubtful cases of the disease appeared in the 66th whilst under canvas, and decided cases of it soon after their entering the stone and brick barracks. The disease did not spread or increase regularly; there were pauses, when it seemed to be subsiding. Thus, whilst in April, four men of this corps, according to the returns, died of it,—in May it proved fatal only to one. In June, July, and August, its increase, as marked by the mortality, was more regular, in the first-named month three having died of it; in the second five; and in the third six, including two men not of the corps from the adjoining dock-yard barrack, Europeans. In September, when the disease was decidedly on the increase, with increased mortality, the stone and iron barracks were again vacated, and the men were encamped on the ground behind the brick barracks, in about the same situation as the 88th had been located on the like emergency.

From the first week of April to the 1st of October, the deaths in the 66th from fever amongst the men, about 400 strong, were 33; in the Royal Artillery, about 155 strong, the deaths were 2; whilst in the 72d Highlanders, about 610, there were none.

After the first week in October, the disease had greatly abated in the 66th; and about the same time it broke out and became fatal in the 72d, and more frequent and destructive than before in the Royal Artillery. Thus, from the 5th of October to the 7th of November (beyond which I have no accurate returns, in consequence of my departure from Barbados for England), the deaths from fever in the 66th were none; in the 72d, exclusive of officers, they were 10, and in the Royal Artillery 6.

Shortly after my leaving the island, the 72d regiment was also placed under canvass; the detachment of the Royal Artillery had been so before; the latter in a spot close to the artillery huts on the savannah, the inmates of which had been exempted from fever previously; the former within the inclosure of the old naval hospital, about a mile from the garrison, where, after a time, but not without considerable further mortality, the disease ceased to appear.

From the information I have received from Barbados, the total deaths from this fever up to the 8th February of the present year, amongst the troops in garrison there during fourteen months, were about 196, viz. 16 officers and 180 men, and out of a force little exceeding 1200.

The nature of the disease was well marked. The symptoms generally, especially in the severer cases, were those indicative of the malignant yellow fever. The most characteristic associated with the peculiar yellowish tinge of the skin were great irritability of stomach, difficulty of retaining anything swallowed, tendency

to passive hæmorrhage in the mucous membranes, especially of the *primæ viæ*; and, in the aggravated instances, the expulsion by vomiting of a blackish turbid fluid containing blood-corpuscles, darkened and altered in form by the action of an acid on them in the stomach. This was proved by microscopical and chemical examination. Thus the fluid vomited strongly reddened litmus paper, and carbonated alkali mixed with it effervesced, and the blackish matter suspended in it, seen under the microscope, exhibited an appearance very like that observable in the blood-corpuscles in a very dilute acid, and was found like them to yield, on incineration, a notable proportion of the peroxide of iron.

During the progress of the endemic, the symptoms seemed to vary more in degree than in kind. At the more advanced period, the disease, as it appeared in the 66th regiment, seemed to be less intense, of a less malignant character, less prone to passive hæmorrhage, than in the earlier period, and more protracted and less fatal. In some instances, it had a good deal of the character of the common continued fever.

With the symptoms above mentioned the results of the *post mortem* inquiry accorded, and it was instituted in the great majority of cases that proved fatal in hospital. In no tissue were there any marks of inflammation; in no organ any well-marked lesion, excepting perhaps occasionally in the stomach, in an appearance of slight abrasions or of minute spots of ulceration. The vessels, especially of the mucous membranes, and more especially of the stomach and intestines, were turgid and discoloured, often in patches with dark blood. The lower parts of the lungs, and also the integuments of the inferior parts of the body, were similarly turgid and discoloured. The vessels of the brain were commonly loaded in the same manner; no appearance of lymph was discovered on this organ, and but little more serum than usual. The white parts, especially the skin and the coats of the arteries, were in most instances more or less yellow. The liver, too, was generally yellowish, the colour nearly of unbleached wax, and its substance, it was thought, was more friable than usual. The bile was more or less viscid, sometimes green, and the common duct was sometimes clogged with mucus. The blood in the heart and great vessels was generally thick and dark, coagulating either not all or imperfectly, and in several instances it was found acid by the test of litmus paper.

A like accordance, too, was observed at the later period of the disease. When the fever had more the character of the common continued kind, then the blood after death was often found not acid, nor in the liquid or loose state, but coagulated pretty firmly; and, in the same instances, the integuments and white tissues were little if at all tinged yellow.

It was a desideratum to ascertain the state of the blood during life in the well-marked cases of the disease. In three different examples, in which, in the advanced or last stage of the fever, a minute quantity, a drop or two, was taken from a vein and put into a strong solution of common salt, the corpuscles, under the microscope, exhibited their normal form, and about their normal dimensions, excepting in two, in which they seemed thinner than usual. Whenever the blood was examined taken from the dead body, the corpuscles were seen to be corrugated, as if from incipient putrefaction, to which, after death, and even before, in the advanced stage, there was a great tendency.

Some inquiry was made by Staff-surgeon Dr Collings, after I left Barbados, into the state of the urine. He found it in a large number of cases albuminous, as he has informed me by letter, and accompanied, in the fatal cases, by a condition of the kidney similar to that witnessed in Bright's disease,—a condition of the secretion and of the organ, I may remark, according with the state of the mucous membrane of the *primæ viæ*, and with the passive hæmorrhage to which it is subject, as did also the petechiæ, which, in a few cases, appeared on the skin.

Relative to the origin of the endemic, it must, I believe, be confessed, that there was the greatest obscurity. The weather, at its commencement, was unusually cool and pleasant; before its outbreak, and whilst it lasted, the seasons were nowise peculiar. Several circumstances at the time were pointed out as probable causes,—such as an accumulation of rank vegetation on, with urinous exhalation from, a neglected hollow between the artillery and stone barrack, and to windward of the latter; the state of the savannah, and of the adjoining ground defectively drained, liable to be flooded after heavy rains, to retain water stagnant for a while, and, after a period of drought, to become parched and fissured; the state of the grave-yard, lying between the barracks and the shore, in unsuitable ground, either too rocky generally, or sandy and unduly crowded; and, lastly, the breaking up of a considerable extent of ground to some depth, and the levelling it for the improvement of the works in the neighbourhood of the barracks, where the disease first appeared. Whether any one of these circumstances, or whether all of them combined, had effect or not in the production of the disease, I cannot venture to affirm. That it was of local origin, seems to me hardly doubtful, whatever the local cause may have been. The fact most conclusive in proof of this was the isolation of the disease. During the whole period, it was confined to the garrison of St Ann, and, during a considerable portion of the period, to the barracks and quarters situated on the lower ground, where the drainage was most defective, and the conjectured local causes most active.



An opinion, I am informed, has been expressed, that the disease was imported, and by H. M. war-steamer, "Growler," from the coast of Africa. That ship was employed in conveying liberated Africans to the West Indies. She left Sierra Leone on the 12th of November 1847, and arrived at Trinidad on the 5th of December. During the voyage 46 deaths occurred amongst the emigrants from chronic dysentery, and 2 deaths amongst the crew from fever. The emigrants were landed at Port of Spain. No fever was there introduced, although no precautions were taken that I have been able to learn, to prevent unrestricted communication. She took her departure from Trinidad on the 8th of December; arrived at Barbados on the 10th; proceeded from thence on the 18th, and reached the Bermudas on the 24th. There, according to established usage, having come from the coast of Africa with fever on board, she was placed in quarantine; the crew landed and kept apart,—the sick from the well,—and the vessel thoroughly cleaned and fumigated. On examining the holds and bilges they were found in a very offensive state, from accumulated vegetable matter, rice, chips, and shavings in a decomposing state. "The atmosphere in the carpenter's store-room would not support combustion." The fever was not communicated at the Bermudas to a family of five persons associated with the sick on shore; but two individuals from the shore who were engaged on board in conducting the cleaning of the vessel contracted it, as did also a certain number of the crew similarly employed, and only those so employed. The total number of cases of fever under treatment from the "Growler" were 75, of which 3 only terminated in death, 72 having recovered. These particulars I have collected from an "Extract from the Journal of Mr Robert M'Crae, Surgeon of H. M. S. Growler," a copy of which interesting document is now before me, with which I have been favoured by the head of the medical department of the navy, Sir William Burnett. Mr M'Crae, who had the best opportunities for arriving at the truth, in this Journal expresses himself satisfied that the fever originated in the ship from the causes referred to; and that it was nowise contagious. The facts he adduces seem to bear him out fairly in these conclusions.

But even if these conclusions of his be doubted, there is no proof that the yellow fever in Barbados was derived from the Growler. It was rumoured that the two men of the 88th regiment who were first attacked and who died had been on board. The result of careful inquiry is that this rumour was groundless, a mere conjecture. Even considering the time of arrival of the Growler at Barbados on the 10th of December at noon, and that the first two fatal cases in the 88th terminated on the 19th of that month, one after a treatment of three days in hospital, the

other of five days, it seems improbable that the fever was contracted by a visit to the ship, were there proof, which was altogether wanting, that such a visit had been made. Moreover, the wives of the two men first attacked also experienced the disease, and according to my recollection, for I cannot find any note on the subject, as soon as or a little before their husbands; and they too fell victims to it. They occupied a low, ill-ventilated room of the iron barracks, intended not for the accommodation of troops, but for use as a store-room. It was here that the disease first appeared, and scarcely a person escaped the fever who, from inconsiderate indulgence, had been permitted to be there. They were married men, and their families, to whom any place apart is always acceptable. Further, it is worthy of remark, that the inhabitants of Bridgetown, like the inhabitants of Port of Spain in Trinidad, were not visited by fever after the arrival of the *Growler*; many of whom, in communication with the crew in affording supplies, must, it may be inferred, have been more likely to have contracted it, had the disease been contagious, than the troops in garrison. Even the character of the fever amongst the crew of the *Growler* and of that in the 88th had not such a close resemblance as to warrant their being considered identical in species. Yellowness of the skin is not mentioned as having appeared in any instance amongst the former. The majority of the ship cases appear to have been of no great severity, and without the symptoms denoting the malignant character,—a mildness well marked by the large proportion of recoveries, 1 only in 25 proving fatal, whilst in the garrison 1 died in about every 5 attacked. It is true, that in some of the *Growler* cases there was irritability of stomach, and that, in one which terminated fatally, death was preceded by “black vomit,” a solitary example as reported by the surgeon. But irritability of stomach is not an unfrequent accompaniment of the common remittent fever of the West Indies, and “black vomit” is occasionally witnessed in it, or in sporadic cases of fever, which may be of the nature of yellow fever, although solitary,—viz. the symptoms of the two—of yellow fever and of remittent, excepting when the diseases are strongly marked, having many points in common, and consequently difficult to be distinguished.

The same persons who suppose that the fever was imported, of course also suppose that it was contagious or infectious. Whether yellow fever is contagious or not seems to be admitted to be a difficult problem. The manner in which those who have specially considered the subject are divided in opinion respecting it, is sufficient proof of this difficulty. I feel it right, candidly to confess, that at the outbreak of the disease I belonged to the class of non-contagionists; and that what I witnessed during its

progress confirmed me in the persuasion. The fact of its being confined for so many months to the low localities in which it first appeared; not spreading to the troops occupying the higher barracks, though not more than 1600 feet distant; and not spreading to them even in the instance of the 7th Royal Fusiliers when the 88th were encamped within a few feet of the upper barracks, and for some time were sending into hospital fresh cases of the disease; the manner in which the inhabitants of the populous and crowded town adjoining, Bridgetown, in unrestricted communication with the garrison remained exempt from it; the small number of cases of the disease which originated in the hospital, whether amongst the orderlies attending on the sick, or amongst the sick themselves, such who were admitted with other ailments, —are circumstances, it seems to me, not reconcilable with the idea of the fever being of a contagious character. I have alluded to the small number of cases of fever that originated in hospital. From a return now before me, it appears, that from the 26th February to the 30th of September 1848, in the 66th hospital, one orderly only attending the sick was attacked with fever, and that slightly, and only three patients admitted with other diseases.

Some respectable medical authorities hold a doctrine, as it were intermediate between that of contagion and of non-contagion, viz. that a disease not originally contagious may become so under peculiar circumstances. To prove or disprove this is necessarily difficult. It must be kept in mind that every endemic or epidemic disease has, as to the manner of its occurrence, after its first appearance, a good deal of the character of an infectious or contagious disease. Every disease of the former kind has, at some time or other, been held to belong to the latter. Those who advocate this expediency-doctrine might adduce in support of it the circumstance that, though the 7th Royal Fusiliers, in communication with the 88th, escaped the fever, and though the 72d Highlanders, for some months similarly situated, also escaped it;—yet ultimately the disease appeared amongst them, and after its first appearance spread rapidly. In reply, it may be remarked, that, considering how near the regiments were stationed in the same garrison, living in the same manner, performing the same duties, and often in common, and exposed, therefore, partially to the same causes, it is more surprising that the 72d so long remained exempt, than that it was at length attacked.

The history of yellow fever in the West Indies, from the early period, when, by the contagionists of that time, it was called the "Mal de Siam," to the later period when it was named the "Boulam fever," seems to me to afford corroborative evidence, that, in every instance, it has been of local origin, and neither contagious at its commencement nor in its progress. From all the information I have been able to collect, yellow fever is not the

prevailing fever of the western coast of Africa ; it is doubtful even whether it ever occurs there. Of this we are certain, that it rarely, in the West Indies, attacks individuals of the African race. We are also certain that those situations most exposed to infection or contagion from without, such as ports communicating most freely with other ports and marts of commerce, are not more frequently visited by yellow fever than spots where there are troops in a manner isolated. I think, too, it must be admitted, that, in no instance, not a single one, not even in that specially called the Boulam fever, has there been fair and satisfactory proof afforded that the disease was imported and not of local origin.

To enter into details on this subject would be here out of place. I may have occasion to revert to it shortly in editing a monograph on yellow fever, intrusted to me for publication by my friend Dr Blair, Surgeon-General in British Guiana, in which much valuable information, the result of his experience in that malarious country, has been collected.

There are other questions of much importance which I must pass by, the discussion of which would occupy too much space at present, such as, whether the yellow fever of the West Indies and the remittent fever are varieties of the same disease, or distinct species ; and what are the modes of treatment which have been most successful in yellow fever ; not to mention some other points not without interest, whether practically or theoretically considered.

*Lesketh Howe, Ambleside,  
7th July 1849.*

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ART. III.—*Report upon Cholera as it appeared in the 17th District, of the City Parish of Glasgow during the months of November, December, January, February, and March 1848–49.* By ALEX. MAXWELL ADAMS, M. D., L. R. C. S. E., Professor of Institutes of Medicine, Andersonian University.

MALIGNANT cholera having twice invaded this country within my own recollection, and there being every probability that the recent visitation is not its last, I therefore conceive it incumbent upon every one whom circumstances have placed in a position to witness the phases and symptoms of this extraordinary malady, and the effects of the remedial agencies employed for its prevention or removal, to record his observations, in order that combined and extensive experience may lead to more perspicuous and definite views regarding its nature and treatment. The position which I occupied as medical superintendent of one of the paro-

chial districts, and as physician to one of the cholera hospitals in Glasgow, necessarily gave me considerable experience, and afforded me favourable opportunities for such purposes. I have tabulated the results in such a form as to furnish elements which, when combined with those supplied by the experience and observation of others, may afford data for important generalizations of sound and practical application. I am aware that the number of cases treated and witnessed by me was much too limited to render unquestionable all the inferences which I have considered deducible from the facts referred to. Any misgiving, however, on this ground, cannot detract from the importance of the facts themselves, and it is for this reason that I lay them before the profession. And although they are not put forth entirely naked, they will be found so thinly surrounded with speculation, that no one who considers them will be in danger of losing sight of them by reason of the attractive garniture of abstruse hypotheses.

The parochial district of which I have the medical charge is situated on high ground in the north-western suburbs of Glasgow, and contains a population of about 10,000 persons, a considerable proportion of which is composed of the poor and working classes. The Forth and Clyde Canal runs through a part of the district, and the houses in which the disease first appeared were situated on the banks and below the level of this artificial water-way. They are consequently damp; and the dung-steadings in connection with them, or in their immediate proximity, are in some instances half-full of stagnant water, which has percolated through the soil intervening between them and the canal. In addition to this source of miasmata and unhealthiness, there are several worked-out quarries, filled almost in every instance with water of a more or less turbid and stagnant description; but what is still worse, a burn runs through one part of the district, which conveys the refuse matters from certain chemical and other works in the neighbourhood, and which is constantly discharging immense volumes of deleterious gases, such as sulphuretted hydrogen, &c., which are felt to impregnate the atmosphere for a considerable distance around its locality. Many of the houses are very old, having no water-closets or other such conveniences; and, instead of being furnished with jaw-boxes inside, these are placed in the entrance stair-window of each general landing-place. The householders, in order to save themselves the trouble of going down stairs, are in the habit of throwing filthy substances into these sinks, insomuch that the fumes emitted from them are often so offensive that the uninitiated are frequently compelled not only to turn up their noses against their reception, but often to reject the contents of their stomachs along with what they could not help having imbibed.

From the preceding description, it cannot be matter of surprise to any one who has paid attention to the sanitary condition of large cities, that epidemic and contagious diseases should be attracted to so favourable a locality; and such has hitherto been the fact not only in the late but in previous epidemics. Here these disorders commence their first ravages; here they linger longer than in any other district of the city; and here they are more fatal in their effects. Such results, although not exactly in accordance with *à priori* expectations, considering the suburban and elevated position of this district, yet are perfectly reconcilable with the local, and, in some instances, removable peculiarities to which I have adverted. It was a knowledge of these circumstances which made me venture to predict to parties in authority, previous to the invasion of cholera, that, should it come to Glasgow, this particular portion of the city would, in all probability, be the first to be attacked, or, if not, that its immunity would be of a very temporary character, and its mortality in either instance very great. This prediction, founded as it was upon my experience of former epidemics, can scarcely be called a mere guess, and, to me at least, it was no matter of surprise to find it verified to the letter; for, with the exception of one case imported from Edinburgh, on or about the 5th of November last, to a house at Garngad Road, between one and two miles from my district, no cases occurred until the 11th of the same month, when the disease broke out simultaneously in it, and in the adjoining one situated in the Barony Parish. Of the favourable opportunity thus afforded me for tracing its spread and progress, I carefully availed myself, and the results cannot be deemed uninteresting by either contagionist, non-contagionist, or contingent-contagionist, as they furnish materials for the comments of these conflicting classes. Before, however, entering upon disputed ground, I will direct attention to the various forms of choleraic diseases which appeared, and make a few remarks upon the symptoms and circumstances connected with them as they came under my observation.

The table No. 1 (p. 288) exhibits a total of 1227 cases of what I believe to have been choleraic diseases. My reasons for this opinion are, that diarrhoeal affections do not, as was the case in this instance, prevail during the winter months; that they commenced with the invasion and declined with the disappearance of the pestilential cholera; that 9 of them, notwithstanding active medical treatment, lapsed into this disease; and that many of them occurred in the same localities, tenements, and houses in which it prevailed. Seeing that I view the several affections enumerated in this table as varieties of the same species of disease, it may be asked, to which of them do I give the name of malignant cholera? I would say, to the third. But in the table this term is not adopted, be-

No. I.—Analytical Table of the Choleraic Diseases occurring in the 17th District of the City Parish of Glasgow from 11th November 1848 till 6th March 1849, distinguishing those treated in their own houses from those treated in hospital.

Classification of the most prominent symptoms.	Treated in their own houses.												Treated in hospital.												General total.															
	Nov. from 11.			Decemb.			January.			February.			Mar. 1st till 6th.			Total.			November.			Dec.			Jan.			Feb.			March.			Total.			Treat. at home & in hospital.			Centes. report of deaths at home and in hospital.
	Recov.	Died.	Total.	Recov.	Died.	Total.	Recov.	Died.	Total.	Recov.	Died.	Total.	Recov.	Died.	Total.	Recov.	Died.	Total.	Recov.	Died.	Total.	Recov.	Died.	Total.	Recov.	Died.	Total.	Recov.	Died.	Total.	Recov.	Died.	Total.							
1. Simple diarrhoea .....	48	48	200	269	269	149	16	682	682	1	1	4	4	2	2	5	5	687	687	1	1	4	4	2	2	5	5	687	687	1354	1354	2	2	196	196	687	687			
Diarr. without bilious or fe- culent matters in evacua. }	8	8	70	72	95	95	19	192	2	194	1	1	4	4	2	2	5	5	687	687	1	1	4	4	2	2	5	5	687	687	1354	1354	2	2	196	196	687	687		
Total.....	56	56	270	272	364	364	168	168	16	874	2	876	1	1	6	6	7	7	881	2	883	0	2	883	0	2	883	0	2	883	0	2	883	0	2	883	0	2	883	
2. Bilious purging with vomit. Bilious purging with spasms. Bilious purging with both.....	1	1	3	3	1	1	2	6	6	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		
Total.....	8	8	11	11	4	4	5	2	30	2	30	2	30	2	30	2	30	2	30	2	30	2	30	2	30	2	30	2	30	2	30	2	30	2	30	2	30	2	30	
3. Serous purging with vomit. Serous purging with spasms Serous purging with both ...	3	3	14	22	9	31	7	14	5	4	9	9	37	31	68	2	1	3	4	1	5	2	4	8	4	12	45	35	80	3	3	3	3	3	3	3	3	3		
Total.....	11	25	36	46	55	101	20	25	54	20	13	33	1	1	107	118	225	9	11	20	19	23	42	7	5	12	35	39	74	142	157	299	142	157	299	142	157	299		
General total.....	75	25	100	327	57	384	897	25	422	198	13	206	19	19	1011	1201	1131	12	12	24	30	25	55	12	5	17	54	42	96	1065	162	1227	1065	162	1227	1065	162	1227		

cause I have found among my medical friends considerable discrepancy as to the combination of symptoms which they considered essentially necessary for the identity of a case of genuine cholera, and because I thought it better to tabulate the cases and symptoms in such a manner as to permit others to name them as they might think proper. Few, if any, will refuse assenting to the proposition, that, when any of these affections prove fatal during an epidemic of cholera, such cases ought to be classed amongst the true instances of the disease. In the table, it will be seen that two deaths occurred in class No. 1 of the cases treated at home, and three in class No. 2 of the cases treated in hospital. Of the first, one was a feeble infant, the other a frail old woman. Both sunk gradually without any other symptoms beyond those enumerated than a diminution in the quantity of urine. Of the latter deaths, I can only say that the patients had not been brought to hospital until they were in an advanced stage of the disease; and I have reason to believe that the characteristic serous evacuations had been present at the commencement. I could not, however, introduce into the table as certain, symptoms for the presence of which I had no direct or tangible evidence.

Notwithstanding these five deaths, it is a remarkable fact, that mortality was a result almost exclusively attaching itself to the cases in class No. 3, and this is the reason why I look upon them as examples of the fully-developed disease, and why in my subsequent tables and remarks I throw out of consideration the others.

By many the suppression of the urinary secretion is considered as marking, in an especial manner, malignant cholera. Those who do so will no doubt consider the table faulty, inasmuch as it makes no mention of this symptom; but the facts are, that in many instances there were no means of ascertaining whether or not the contents of the bladder, if any, had been evacuated at the same time with those of the bowels; and farther, that in some few cases death ensued, notwithstanding that the kidneys performed their functions nearly to the last, although certainly to a more limited extent than usual. Seeing that I could not, without the risk of error, introduce this symptom into the table, and having ascertained that the presence of the true disease was not incompatible with the power of secreting urine, I thought it would answer every purpose merely to state here, in general terms, that the action of the renal organs was temporarily, if not entirely, lost in an immense majority of cases, and diminished in a marked manner in the remainder.

As I do not mean this report to embrace a full and detailed description of the varying symptoms which characterized individual cases, I will merely state what were the most general, and then make a few comments upon their relative frequency and



value, as diagnostics in marking the stages and other peculiarities connected with the disease.

Usually the first symptoms were flatulence and uneasy sensations in the bowels. To these succeeded diarrhœa, feculent at the onset, and subsequently serous; then vomiting and spasms came on almost simultaneously. At this stage, the pulse fell in force, and generally in frequency. Thirst was urgent; the tongue became cold, and was invariably moist. The surface of the body also grew cold; the hands, fore-arms, feet, and around the eyes, assumed a livid appearance. The features became collapsed, and the expression anxious. The urine was suppressed. The voice got weak and husky, and, at a period more or less remote, according to the nature of the attack and the strength of the patient, the evacuations from the stomach and bowels, together with the spasms, subsided, and death closed the scene. In more than one-half of the cases, however, the patients rallied. The pulse, if previously absent, returned; the skin became warm; the kidneys resumed their functions, and, at an indeterminate period, feculent stools were evacuated, which were at first of a pitchy dark colour. Recovery then took place either immediately, or not till after the lapse of a few days, in which case the irritability of the stomach and bowels continued, and was often associated with some slight degree of fever. In some of these cases, vomiting and purging of bilious matters supervened, and continued to such an extent, as to lead to a secondary collapse, and not unfrequently death. In others, there was a complete cessation of the gastro-intestinal irritation, but the urinary secretion remained either totally suspended, or reduced considerably below the natural standard. On this state of matters, a semi-comatose condition usually ensued, from which the patient could be but partially roused, and then he presented very much the appearance of one labouring under typhus fever, excepting that the pulse was in general fuller and less frequent, sometimes even slower than in health, and seldom above 80; that there was little or none of the muttering delirium usual in that disease; and that there was a complete absence of the characteristic maculæ.

I will now consider the symptoms to which I have thus cursorily referred *seriatim* :—

1. *Diarrhœa*.—This is the most essential symptom of the disease as witnessed by me. In no case was it absent. At first the stools were feculent, they afterwards became watery, sometimes appearing like water into which a little meal or bran had been thrown, which had mostly subsided to the bottom of the containing vessel; at other times they were quite limpid. This was the appearance in the worst cases.

The bran, like sediment in the former instance, seemed to be

composed to a great extent of epithelial scales, and the limpid fluid in the latter of the purely aqueous portion of the *liquor sanguinis*; I cannot say whether or not it contained salts, but certainly it contained neither fibrine nor albumen in any appreciable proportions. To detect the latter principle, I boiled the fluid and treated it with nitric acid, but always with a negative result.

A question arises here in reference to diarrhœa, which is of consequence from having a direct bearing upon certain prophylactic and curative measures, to be mentioned further on. The question is this,—is cholera invariably preceded by diarrhœa? Many affirm that it is, but a reference to the succeeding table, on page 292, will prove this opinion to be altogether untenable.

This table contains a further analysis of the 225 cases of class No. 3 referred to in the first table as having been treated at home. These are selected, because no one can for a moment doubt that they were genuine examples of malignant cholera, and also because particular care was taken to ascertain the existence or non-existence of premonitory diarrhœa prior to the supervention of graver symptoms. In 116 cases this symptom was either altogether absent as a premonition of the disease, or, if present, it was for a less period than four hours. In such the mortality was considerably greater than in those in which there was premonitory diarrhœa. In the former it was as high as 62 per cent., and in the latter 42·2. This goes to prove that the greater or less fatality of the results is commensurate with the greater or less suddenness of the attack. Another interesting feature which this table exhibits is, that at a very early and at a somewhat advanced period of life,—those periods, in short, when the system is less able to bear up against the influence of depressing agents,—the per centage of sudden attacks was very high.

The next table, on page 293, is constructed so as to show the number of cases with and without premonitory diarrhœa occurring *each month* from the commencement to the close of the epidemic.

This table is very interesting, showing, as it does, that diarrhœa of a premonitory character was of less frequency at the earlier than at the latter periods of the epidemic. In November, for example, the centesimal proportion of cases, without premonitory diarrhœa, was 80·5; in December, 44·5; in January, 51·6; and, in February, 39·4. This proves one of two things,—either that the epidemic influence was more intense at first, or that it affected, primarily, individuals predisposed in a remarkable degree for its reception. The latter hypothesis is borne out by the slight increase in the centesimal proportion of the sudden seizures which happened in the month of January. It is well known to all acquainted with the habits of the Scotch people, that, about the

No. 2.—Tabular statement of the number of cases of serous purging, with vomiting or spasms, or both, in which the diarrhoea was of more than four hours' duration prior to the supervention of other symptoms, and of the number in which there was either no premonitory diarrhoea, or in which its duration was less than four hours.

Classification of the Cases.	Aged under 10 years.				10 years, and under 20.				20 years, and under 30.				30 years, and under 40.				40 years, and under 50.				50 years, and under 60.				60 years, and upwards.				Total.				Centesimal proportion of deaths.								
	Rec.		Died.		Rec.		Died.		Rec.		Died.		Rec.		Died.		Rec.		Died.		Rec.		Died.		Rec.		Died.		Total.												
	M		F		M		F		M		F		M		F		M		F		M		F		M		F		Total.												
	Total.		Total.		Total.		Total.		Total.		Total.		Total.		Total.		Total.		Total.		Total.		Total.		Total.		Total.		Total.												
Diarrhea, existing for more than four hours prior to the occurrence of other symptoms .....	4	2	...	2	8	3	2	2	4	11	3	7	4	18	13	12	5	7	37	5	6	4	5	20	1	2	3	...	6	2	1	...	6	9	31	32	18	28	109	42.3	
Diarrhea, either non-existent before other symptoms, or lasting for less than four hours .....	3	5	9	4	21	3	10	5	2	20	7	1	5	8	21	4	2	3	10	19	3	...	4	9	16	2	2	3	1	7	1	1	3	7	12	23	21	31	41	116	62.0
Total.....	7	7	9	6	29	6	12	7	6	31	10	8	9	12	39	17	14	8	17	56	8	6	8	14	36	3	4	5	1	13	3	2	3	13	21	54	53	49	69	225	52.4

*Note.*—This and the next table include only the cases treated in their own houses, there having been 74 of the same class treated in hospital, relative to whom the same particulars as regards the existence or non-existence of premonitory diarrhoea could not be ascertained with the same accuracy.

No. 3.—Tabular statement of the number of cases of serous purging, with vomiting or spasms, or both, in which the diarrhoea was of more than four hours' duration prior to the supervision of other symptoms, and of the number in which there was either no premonitory diarrhoea, or in which its duration was less than four hours, constructed so as to show the relative proportion of each occurring monthly.

Classification of the Cases.	November, commencing on the 11th.			December.			January.			February.			March, from 1st to 6th.			General Total.			Centesimal proportion of deaths.							
	Rec. Died.			Rec. Died.			Rec. Died.			Rec. Died.			Rec. Died.			Rec. Died.										
	M	F	Total	M	F	Total	M	F	Total	M	F	Total	M	F	Total	M	F	Total								
Diarrhœa, existing for more than four hours prior to the occurrence of other symptoms.....	2	1	4	7	17	15	10	14	56	5	10	5	6	26	7	6	3	4	20	31	32	18	28	109	42.2	
Diarrhœa, either non-existent before other symptoms, or lasting for less than four hours.....	5	3	12	9	29	9	5	11	20	45	5	9	5	9	28	3	4	3	13	1	23	21	31	41	116	62.0
Total.....	7	4	12	13	36	26	20	21	34	101	10	19	10	15	54	10	6	7	33	1	54	53	49	69	225	52.4

Christmas and New Year times, they indulge to a great extent in the use of alcoholic liquors ; and I shall subsequently show that intemperance predisposes, in a marked manner, to an attack of cholera in its most aggravated form. It is in this way, therefore, that I account for the rise in the proportion of sudden seizures which the January cases indicate.

2. *Urinary Secretion.*—I have already stated that this was suppressed in the great majority of instances, but as some proved fatal, in which it continued to some extent up to the last, its absence must be viewed as a less essential phenomenon than the one we have just been considering, but, next to that, it is of the greatest importance. In some cases, I have known it to have been completely suspended for more than three days without death ensuing. This, however, was by no means of great frequency. Generally, when the patients rallied out of collapse, and the urine continued suppressed, a *semi-comatose* condition came on, followed in every instance, when well marked, by death. My belief is, that, in all these cases, the coma depended on the non-elimination of urea by the kidneys, and that this principle, circulating in the blood, acted as a narcotic upon the nervous centres. This opinion derives corroboration from the fact, that, in two cases of this description, although urine continued to be secreted, yet, on analysis, it was found to be deficient in the normal proportion of urea.

I was afforded an opportunity of making a *post mortem* examination in only three cases, where the patients had died comatose, but in none of them did I detect anything beyond great congestion of the cerebral veins and sinuses, the same appearances, in short, which are found in cases of narcotic poisoning, and of *ischuria renalis*.

When recovery occurred after complete suppression, the kidneys on resuming their functions secreted urea in larger proportions than usual. In this respect the analogy between them and the liver was very complete ; for this organ also, when it commenced to act again, did so with increased activity. Urea in the urine, and bile in the alvine evacuations were therefore two appearances always associated together. Very frequently after the re-establishment of the urinary secretion albumen was detected in this fluid, but it soon disappeared. The frequent presence of this principle under such circumstances led me to conjecture that the epithelial cells of the convoluted *tubuli* of the kidney, whose function it is to abstract from the blood the solid constituents of the urine, were increased in numbers in correspondence with the increased amount of work which they were called upon to perform,—that the *tubuli* became distended with their contents,—that the efferent vessels of the Malpighian *glomeruli* were thereby com-

pressed,—that the *glomeruli* themselves, owing to this, became congested, and that albumen then transuded just as happens when the renal veins are tied in animals, or when they are compressed by a tumour, or by the gravid uterus, as sometimes takes place in pregnancy. I regret having neglected to examine the sedimentary portion of the urine in these cases with the microscope; for the hypothesis which I have ventured to propose would have been greatly strengthened had I found by this means of investigation a superabundance of epithelial cells in this fluid.

3. *Vomiting*.—This symptom is the next in frequency to those commented on, and therefore demands consideration next in order.

The table, page 296, speaks for itself, and almost precludes the necessity of any remarks in so far as vomiting is concerned. It shows that in 26·7 per cent. of the cases no other symptom but this was conjoined with the serous purging, excepting more or less suppression of urine, of which the table takes no note. It shows also that the greatest proportion of these cases was in individuals under 10 years of age; and further, that the mortality was on the whole much less than when spasms came to be superadded. On the one hand, we perceive the deaths to be at the rate of 43·7 per cent., whilst on the other it rises as high as 56·5.

4. *Spasms*.—From the same table it appears that there were only three cases in which spasms constituted the only symptom in connection with serous purging. None of these cases proved fatal, but from concomitant circumstances I have no hesitation in classing them in the category of malignant cholera. It is a very remarkable circumstance, and one which I do not pretend to explain, that children under 10 years of age had seldom spasms, and when they had, which was only in three instances, they were of so slight a nature, and so transitory in duration, as scarcely to deserve notice.

I consider this the proper place for mentioning, that in four cases, when the patients had convalesced so far as to be able to move about, they were suddenly seized with violent spasms, which were of a tetanic character, and affected nearly all the muscles of the body, including the masseters; under appropriate treatment, however, these symptoms subsided as suddenly as they came on, and left no ill consequences behind them.

To prevent misconception, I will sum up in a few words what has been stated in reference to the symptoms just specified. In all the cases included in class 3 of table No. 1, *i. e.* all the cases which I have considered as genuine or malignant cholera, there was purging of a serous character. In the majority there was more or less suppression of urine. In 80 cases these symptoms were conjoined with vomiting alone, in 3 with spasms alone, and in 216 with both.

No. 4.—Table showing the most prominent symptoms in connection with cases of serous purging, the ages of the parties attacked, with the results, distinguishing those treated in their own homes from those treated in hospital.

Classification of Ages.	Treated in their own homes.						Treated in hospital.						General total.			Centesimal proportion of cases.	Centesimal proportion of deaths.										
	Ser. purg. with vomiting, spasms.			Serous purging with both.			Ser. purg. with vomiting, spasms.			Ser. purg. with both.			Treated at home and in hospital.														
	Recov.	Died.	Total.	Recov.	Died.	Total.	Recov.	Died.	Total.	Recov.	Died.	Total.	Recov.	Died.	Total.												
Under 10 years.....	12	14	26	...	...	...	2	1	3	14	15	29	4	2	6	2	1	3	6	3	9	20	18	38	127	47.4	
10 years, and under 20.....	7	5	12	1	...	...	1	10	8	18	18	31	2	...	2	6	11	17	8	11	19	26	24	50	16.7	48.0	
20 " " 30.....	4	4	8	...	...	...	14	17	31	18	21	39	...	1	...	...	6	6	12	6	7	13	24	28	52	17.4	53.8
30 " " 40.....	7	1	8	...	...	...	24	24	48	31	25	56	...	...	...	...	7	6	13	7	6	13	38	31	69	23.1	44.9
40 " " 50.....	3	4	7	...	...	...	11	18	29	14	22	36	1	...	1	...	5	6	11	6	6	12	20	28	48	16.1	58.3
50 " " 60.....	3	1	4	...	...	...	1	3	5	8	7	13	1	...	1	...	1	3	4	2	3	5	9	18	6.0	50.0	73.3
60 " " 70.....	1	2	3	1	...	...	1	2	3	10	4	10	14	...	...	...	...	1	1	...	...	...	4	11	15	5.0	73.3
70 and above .....	...	...	...	...	...	...	1	6	7	7	1	6	7	...	...	...	...	2	2	...	2	2	1	8	9	3.0	88.9
Total.....	37	31	68	3	...	...	367	87	154	107	118	225	8	3	11	...	27	36	63	35	39	74	142	157	299	100.0	52.5
Total treated at home and in hospital .....	45	34	79	3	...	...	394	123	217	142	157	299	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Centesimal proportion of deaths.....	43.0	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...

No. 5.—Table showing the state of the pulse of patients in class 3 at first visit, together with the results, and a classification of their ages, distinguishing those treated in their own houses from those treated in hospital.

[illegible]



5. *Pulse at Wrist*.—As the character of the pulse varied at the different stages of the disease, I only condescend in the table, p. 297, upon the condition in which it was found at or during the first visit. This was made in the great majority of the cases within two or three hours from the commencement of active symptoms.

Let us now see what the preceding table elicits. First we observe, that in 164 cases out of 299 the pulse was *absent*. In 113 it was *feeble*, and in 22 *moderately good*. Secondly, we observe that the mortality was greatest in the pulseless cases, being in the proportion of 81.1 per cent.,—in less proportion when it was feeble, viz. 15.9,—and least when it was moderately good, viz. 13.6.

Before constructing the preceding table I was impressed with a decided conviction that a greater relative proportion of pulseless cases occurred under 10 and above 60 years of age than during any other of the decennial periods of life; but this is not in accordance with truth; and I mention the circumstance merely to show that general impressions, however strong, when not founded on numerical facts, are not to be relied on. The centesimal proportion of pulseless cases was greatest between 30 and 40, viz. 24.4,—next greatest between 20 and 30, viz. 20.1,—next between 10 and 20, and 40 and 50 (being in both instances in the same proportion), viz. 15.2,—next under 10, viz. 10.4,—next above 60, viz. 9.1,—and least of all between 50 and 60, viz. 5.5. I merely state these as facts, without drawing, and indeed without professing to be able to draw, any conclusions from them.

The following table is introduced here with the object of showing the relation which existed between the state of the pulse and the existence or non-existence of premonitory diarrhœa:—

No. 6.—Diarrhœa existent and non-existent prior to the occurrence of other symptoms in serous cases, viewed in connection with the state of the pulse at time of first visit.

Classification.	Absent.			Feeble.			Moderately good.			Total.		
	Recov.	Died.	Total.	Recov.	Died.	Total.	Recov.	Died.	Total.	Recov.	Died.	Total.
Diarrhœa existing for more than 4 hours prior to the occurrence of other symptoms, ...	7	37	44	46	6	52	10	3	13	63	46	109
Diarrhœa either non-existent before other symptoms, or lasting for less than 4 hours, ..	9	66	75	31	6	37	4	0	4	44	72	116
Total,	16	103	119	77	12	89	14	3	17	107	118	225

Out of the 119 pulseless cases, it appears that 75 had no premonitory diarrhoea, *i. e.* 63 per cent. ; and out of the 106 pulse-perceptible cases only 41 were without this as a preliminary symptom, or 38·7 per cent. These facts may be expressed in the following language. The majority of the pulseless cases had no premonitory diarrhoea, and the majority of the pulse-perceptible cases had.

6. *Secondary Fever.*—This is the last symptom, if it may be so called, on which observations remain to be made. A considerable number of practitioners appear to regard cholera in its active stages as a species of fever,—almost all, so far as I can glean, speak of secondary fever as an inevitable occurrence when the disease does not prove fatal in the stage of collapse, and many of them assert, that the severity of the former holds a direct relation to the intensity of the latter; but nothing that I have seen warrants any of these conclusions. In the first place, I cannot say that the disease was ever ushered in by those general symptoms which are common to all fevers. In the second place, the next table (p. 300) demonstrates, that the largest number of the recoveries took place within eight days; and I may state, that these were kept under my eye for this period, not on account of any secondary fever, but of debility, indigestion, or some irregularity in the action of the bowels. In the third and last place, only 15·2 per cent. of the pulseless cases remained under treatment longer than seven days, whereas 33·3 per cent. of those in which it was perceptible lasted for a longer period than that just mentioned. Of course the severity of the primary symptoms was greater in the former instance than in the latter; and yet, contrary to the opinion of some, as quoted above, the subsequent stages were not prolonged in consequence. I do not wish it to be understood from these remarks that I have seen nothing of what has been called secondary fever; on the contrary, I have seen several cases, but the symptoms were not in all alike, neither was the duration uniform, nor did I fail, as I considered, in always tracing it to some subacute inflammatory affection of the gastro-intestinal mucous membrane, or to some congestive condition of the liver, lungs, or brain, depending (especially in the two latter instances) upon the arrested action of the *tubuli uriniferi* of the kidneys, and the consequent poisonous condition of the blood.

7. *Ages.*—In respect to the ages of the parties attacked, a retrospective glance at table No. 5., page 297, will show that a greater absolute number of seizures occurred between 30 and 40 than at any other age. This may be termed the culminating point, for up to it the numbers regularly increased, and beyond it they as regularly decreased. Even at first view, this would appear to prove that parties at this age were more predisposed to

No. 7.—Table showing the duration and result of the cases in class No. 3, in which the pulse at the wrist was absent, feeble, or moderately good, distinguishing those treated in their own homes from those treated in hospital.

[illegible]

the disease than at any other; but, to render this conclusion certain, it is necessary to compare the number of seizures at each period of life with the total number of inhabitants alive at the same ages, and this will at once show whether or not they were in anything like an exact proportion.

Assuming, then, the population to be 10,000, or thereby, as already stated, I find that, had persons at all ages been equally liable, there would have been 71 cases under 10 instead of 38; 65 between 10 and 20, instead of 50; 66 between 20 and 30, instead of 52; 43 between 30 and 40, instead of 69; 27 between 40 and 50, instead of 48; 14 between 50 and 60, instead of 18; and 13 above 60, instead of 24. These figures, therefore, conclusively indicate that there is a less predisposition to the disease under 30 than above it, and that the greatest liability is between 30 and 50. I will not hazard any speculations as to the cause of this increased susceptibility, but rest satisfied with simply stating the fact.

It is rather a notable circumstance, that the greater or less predisposition at different ages is not an exact index to the rate of mortality. For instance, this was least between 30 and 40, an age which, both absolutely and relatively, furnished the largest number of cases; but as sex seemed to have some influence in determining this rate, attention must now be directed towards it.

8. *Sex*.—Of the 299 cases in class 3 of table No. 1., 163 were females, and 136 males. Table No. 5 shows the mortality to have been greater in the one sex than in the other. In the females, for example, it was in the centesimal proportion of 55·2, and in the males, 49·2. The most remarkable circumstance, however, in connection with the higher rate of female mortality is, that it did not hold good at all periods of life. Thus, under 20, the male mortality was in the proportion of 57·5 per cent., whilst the female was only 39·6. On the other hand, between 20 and 50, only 42·3 per cent. of the males died, whilst the mortality in the other sex rose as high as 59·3 per cent. After this period, again, viz. between 50 and 60, the balance appeared to be once more struck in favour of the latter. Are the following inferences not fairly deducible from these premises? First, that the constitution of the male under 20 is less able to bear up against choleraic influence than that of the female at the same age; and, secondly, that she, during her menstruating and child-bearing period of life, viz. from 20 to 50, is more apt to succumb to it than he is during this age. This is in perfect unison with the admitted fact, that *her* health is then not so good, generally speaking, as *his*. At the critical age, too, viz. between 40 and 50, it is notorious that it is very precarious; and a reference to the table demonstrates that the mortality in her case was then im-

mense, being as high as 66·6 per cent. This appears more striking when compared with the male mortality at the same age, for it was only 47·6 per cent.

9. *Predisposition*.—This is capable of being treated of under two distinct heads; first, acquired predisposition, and, secondly, predisposition depending upon some condition of the body, the exact nature of which cannot always be ascertained with certainty. For example, I have seen individuals after fatigue, after fasting, after loss of sleep, after a debauch, after taking cathartic medicines, &c., seized with the disease, in consequence, as I believed, of an acquired predisposition induced by these causes. On the other hand, I have known some members of the same families escaping the disease, whilst others were attacked, although, in both instances, they subsisted on the same kind of diet, worked at the same employments, lived in the same infected locality, came equally in contact with the sick, and appeared to have nearly the same kind of constitution. This showed clearly that there was a greater predisposition in some than in others, and that it did not depend, so far as was ascertainable, upon any avoidable cause. It therefore may, for distinction's sake, be called inherent.

In the next table, p. 303, I have made no attempt to adhere to the distinction just drawn between acquired and inherent predispositions, but have endeavoured to bring out the effects of temperate and intemperate habits on individuals whose usual state of health was in various conditions. The result is rather striking.

It will be perceived that where the previous health was bad, no matter what the habits were, the mortality was very high, and that, when this condition was conjoined with intemperance, the deaths were enormous, being in the proportion of 91·7 per cent. When, on the contrary, the health was good, and the habits temperate, the mortality sunk as low as 19·2 per cent.

10. *Period of Day at which the Patients were Attacked and Died*.—I experienced some difficulty in determining what combination of symptoms were necessary before a patient could fairly be stated to be attacked with cholera. At first, I felt inclined to date the commencement of the attack from the period when diarrhoea came on; but as I have all along, in this report, confined the term malignant cholera to those cases in which vomiting, or spasms, or both came to be superadded to serous purging, I thought it best to assume that the disease only began when this addition took place. This, therefore, is the rule which guided me in the construction of the following table. (See p. 304).

No. 8.—Analytical Table of cases of serous purging, with other symptoms, having special reference to the ultimate results in connection with previous habits, condition of health, and ages.

[illegible]

No. 9. Table showing the period of the day at which the patients in class No. 3 (serous cases) were attacked, and the period at which the fatal results occurred.

Treated in their own homes.			
	Attacked.	Died.	Recovered.
From 8 A. M. till 12 Noon,	35	26	9
" 12 Noon " 4 P. M.,	18	8	10
" 4 P. M. " 8 P. M.,	32	19	13
" 8 P. M. " Midnight,	33	18	15
" Midnight " 4 A. M.,	56	20	36
" 4 A. M. " 8 A. M.,	51	27	24
Total,	225	118	107

It will be seen from this table, that the majority of seizures occurred between the hours of 8 P. M. and 8 A. M., viz. 140 against 85. The number of deaths was also absolutely greatest between these hours, viz. 65 against 53, which, however, bears no proportion to the number of seizures at the same period.

With regard to those treated in hospital, the precise time of seizure could not be ascertained, and the following are the periods of the day at which the fatal results occurred.

From 8 A. M. till 12 Noon,	11
" 12 Noon " 4 P. M.,	2
" 4 P. M. " 8 P. M.,	6
" 8 P. M. " Midnight,	7
" Midnight " 4 A. M.,	8
" 4 P. M. " 8 A. M.,	5
	39
Total attacked,	74
Died,	39
Recovered,	35

11. *Origin and Spread of the Disease.*—It has been already stated that one case of cholera (an imported one) occurred in a distant part of Glasgow some time prior to its appearance in my district; and, in reference to it, it is interesting to know, that, of the numerous parties who came in contact with the individual so affected, both in the house in which he lodged, and in Clyde Street Hospital, to which he was subsequently removed, not one took the disease. But to return to my own cases. Late on the night of the 11th November, two individuals, a male and a female, residing in a ground flat of a damp house bordering on the canal, were seized with severe diarrhoea. Neither of them sought medical advice until the morning of the 13th, at which time I found the man in a state of profound collapse and in a dying condition. The woman, however, kept upon her feet up to the time of his death, when she fell into collapse, and speedily afterwards died. I consider that these two persons were attacked at the

same instant, but that the man went more rapidly than the woman into an advanced stage of the disease, in consequence of a debauch in which he had been indulging on the night of seizure. In neither instance was there the most remote probability of direct or indirect exposure to contagion.

The next case happened on the 17th inst. in a village called Springbank, which is likewise situated in the neighbourhood of the canal, and below its level, but at a considerable distance from where the two former cases occurred. The patient, an adult male, was in tolerably comfortable circumstances, but very dissipated in his habits. He had held no intercourse with those first affected, nor did he even know of their existence.

The fourth case occurred on the 18th inst. in the same village with the last, but at a distance of more than 100 yards from it. In this, as in the preceding case, there was no evidence of contagion.

On the 19th inst. another case happened in the same village, at a distance from the others, and without communication.

On the 20th, three individuals, residing apart from each other, and from those previously affected, were seized with the disease; and, as in the previously-narrated instances, without any intercourse. But why go on particularizing? Suffice it to say, that 21 cases occurred before I was afforded an opportunity of seeing two persons consecutively attacked in the same tenement or after communication with the sick. The 9 cases succeeding to these 2 occurred separately and without exposure; but after this a *few* of the infected tenements furnished more than one example of the disease, and therefore, whether there had been communication or not, contagionists would object to them being brought forward as evidence against their theory. It is, however, a singular circumstance, that there were very few instances indeed in which more than two cases occurred in the same dwelling-house, no matter how great the number of its inmates might be. I repeat that it is singular; for the unaffected were in general greatly depressed in spirits and got very little sleep,—circumstances of themselves sufficient to produce an acquired predisposition for the disease. Why, then, was it that such a limited number of persons so situated were attacked? My conviction is, that it depended upon the constant surveillance which my assistants and I kept over them in order to check the first suspicious symptoms, and upon our recommendations in regard to diet, drink, &c. having been attended to.

I ascertained by careful scrutiny, that, in 87 tenements, only *one* case occurred in each; that in 147 dwelling-houses there were likewise only one case in each; and that 160 persons were affected with the disease without having been directly, or, so far as could be traced, indirectly, in communication with other sick.

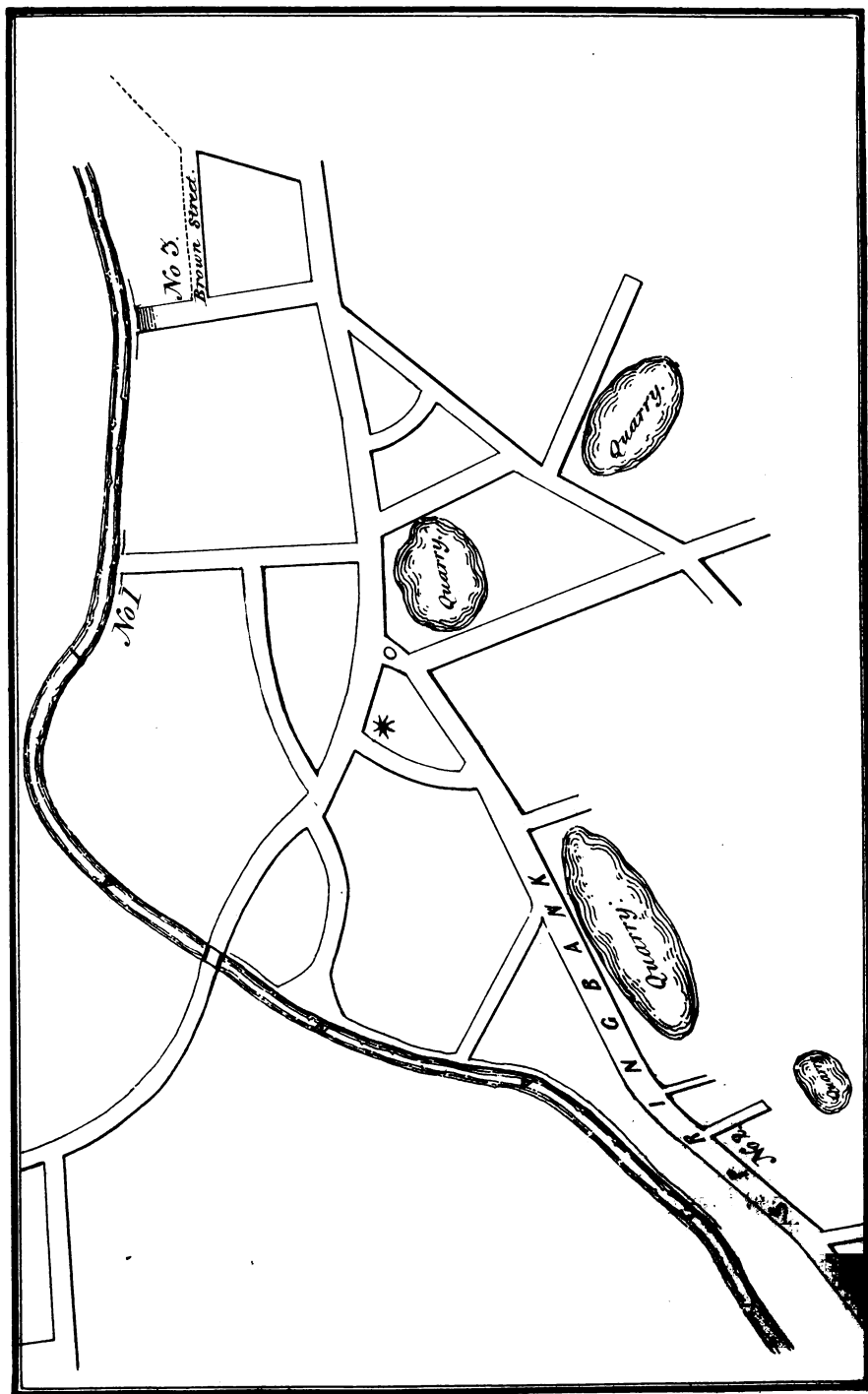


The foregoing facts connected with all that I have observed, have firmly forced on my mind the conviction, that the origin of the disease in my district did not depend upon contagion, but upon some general epidemic influence confined for a time to it and the neighbouring part of the Barony parish. Admitting the disease to have originated thus, a question occurs, did it afterwards become communicable?

Some maintain that it is absurd to attribute the production of the same disease to two different causes,—an atmospheric poison say, on the one hand, and an animal, on the other. But does the supposition involved in the above question necessarily imply that the two poisons or causes must be different? May an individual, or a number of individuals, not imbibe a general poison, and give it off again by the excretions in sufficient quantity to affect others? This is undoubtedly within the limits of possibility, and probably occurs in *some* epidemic diseases; but I am bound to say, that, after the closest attention, I could see nothing connected with the spread of cholera to warrant me in arriving at such a conclusion. The facts coming under my observation, if not in every instance entirely opposed to the preceding theory, yet were so much on the negative side as to force on my mind the conviction, that, if the disease were, under any circumstances, communicable, it was in a very inappreciable degree. For example, when the epidemic was confined to Springbank, many persons living in uninfected neighbouring localities were in the habit of visiting their sick acquaintances with impunity; but much weight cannot be laid on this fact, for, like a two-edged sword, it cuts in different directions.

What can be said of the following truths, however. In 13 instances I knew that relatives lay in the same beds with the sick without being infected. In 9 cases children at the breast escaped the disease, although their nurses laboured under it. Now I would ask, is it at all probable (I had almost said possible) that individuals living in such damp and ill-conditioned houses, no matter how slight their predisposition, could have escaped an attack of small-pox, scarlatina, or typhus fever under similar conditions? I firmly believe they could not. Moreover, were the disease so communicable as some contend,—were the contagious virus given off by the sick capable of penetrating thick stone walls, and of being conveyed in an undiluted form for hundreds of yards through the atmosphere,—why was it that it remained located in my district of the city for fully a month before it extended to the others, and this notwithstanding that no restriction was put on the ingress and egress of inhabitants to and from it? Why was it that one particular corner nearest to the infected locality, inhabited by the most impoverished class of the population, and form-

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ing the direct channel of communication between the place where the disease first appeared, and that where it extended most afterwards, should have entirely escaped? The accompanying map will illustrate this fact better than any language.

The figure 1 indicates the part first affected. The influence, whatever it was, then travelled not by the roads, but apparently by the canal, first, to No. 2, and then to No. 3, in each of which places it committed great ravages. The asterisk points out the corner, which, as already mentioned, entirely escaped.

Lastly, I may ask, why was it that out of 30 individuals employed at Woodside Cholera Hospital in attending to the sick, washing their clothes, and burying the dead, not one took the disease?

This hospital, I may remark, was in consequence of an interdict not opened for the reception of patients until the epidemic had disappeared from the immediate neighbourhood of the locality in which it was situated. If the nurses or others, therefore, had been affected, it would have afforded a strong argument in favour of contagion, for the seizures could not have been with propriety attributed to an epidemic influence, the existence of which, to say the least, was highly doubtful.

I may here sum up the preceding opinions. Every circumstance in connection with the appearance of the malady in the 17th district warrants the conclusion, that contagion had nothing to do with its origin; that its spread depended little, *if at all*, upon it; and that if ever communicable, it was decidedly much less so than any other admittedly contagious disease with which I am acquainted.

12. *Flats of houses in which Cases occurred.*—The propriety of keeping a record of this particular was suggested to me by Dr Sutherland, the representative of the Central Board of Health, London, and I do not regret having done so, as the results are somewhat instructive. It may be premised, that most of the tenements, or lands, as they are named in Scotland, consist of four flats, each containing about an equal number of dwelling-houses. The absolute number of cases, therefore, in the middle flats, should have been nearly equal to that of the other two combined; but this was not the case, for by far the largest number occurred in the ground and upper flats, and the least in the middle. The mortality in proportion to the number of cases was, however, greatest in the latter instance. Why the proportion of deaths should be inversely to the absolute number of cases is a problem for the solution of which I cannot find a key. Had this peculiarity been confined to my district alone, I would have looked upon it as a mere accident; but when I find it corroborated by the experience of other parochial surgeons with whom I have conversed, it must be viewed as a fact, and one of sufficient interest to merit, at least, a passing notice.

No. 10.—Tabular view of the flats of tenements on which the cases treated at home embraced in Classes 2 and 3 of Table 1 occurred.

No. of Class.	Ground Flat.					Middle Flat.					Top Flat.					Total.		
	Recovered.	Died.	Total.	Centes. prop. of cases.	Centes. prop. of deaths.	Recovered.	Died.	Total.	Centes. prop. of cases.	Centes. prop. of deaths.	Recovered.	Died.	Total.	Centes. prop. of cases.	Centes. prop. of deaths.	Recovered.	Died.	Attacked.
2	14	...	14	...	...	5	...	5	...	...	11	...	11	...	...	30	...	30
3	49	57	106	47.1	53.8	26	33	59	21.3	56	32	28	60	26.6	46.6	107	118	225
	63	57	120	47	47.5	31	33	64	25.0	51.6	43	28	71	27.8	39.4	137	118	255

13. *Treatment.*—To the practical physician this is a most important topic, and one to which he is most likely to turn his attention, yet I am obliged to confess with great regret, that my experience of the effects of medicine has been almost entirely of a negative character. I have certainly seen good effects resulting from treatment in the premonitory stage (when there was any) and in that of reaction; but I cannot say that I ever found remedies of unquestionable benefit in that of collapse. In this stage, nature, with as little assistance from art as possible, proved the best physician.

But to pursue some kind of order, I will take up the several choleraic diseases *seriatim*; and, *first*, with respect to diarrhœa alone. This, when simple, was usually treated with sugar of lead and opium, and when of a rice-water character, with calomel and opium. Table No. 1 proves this mode of treatment to have been effective, for only two cases of rice-water purging proved fatal, and only nine lapsed into malignant cholera, notwithstanding the employment of the remedies just mentioned.

*Secondly*, With regard to what I have described as true or malignant cholera. In this form of the malady I essayed the several plans of treatment recommended by the best authorities, but in no instance could I flatter myself with the belief that my efforts had been conducive to the recovery of the unfortunate sufferers.

In all cases I used external heat,—sometimes in a moist form, thinking thereby to supply fluid to the thickened blood; and at other times in a dry form. I occasionally gave calomel in large doses, combined with opium, and afterwards frequently repeated smaller ones with or without opium, according to the intensity of the collapse. In some instances, particularly when the spasms were severe, I abstracted blood from the arm, and employed chlo-

reform inhalations for their relief. When the pulse was absent or feeble I gave as a stimulant, carbonate of ammonia, and I always permitted my patients to drink plenty of fluids, taking care, however, that they did not take too much at one draught.

I consider it unnecessary to burden this report with an enumeration of the other remedies to which I occasionally had recourse; for I never saw them doing any good. I cannot, however, omit mentioning, that many of my patients took no inward medicine at all, and yet that the mortality was not in their cases greater than in those which were treated actively and with unremitting attention. All this leads me to be much astonished when I read of the marvellous effects attending the employment of the same remedies in the hands of others, and tempts me to believe that the disease, as seen by them, was very different indeed from that which it was my painful lot to witness.

I cannot quit the subject of treatment in this the stage of collapse without making a few observations upon the effects of alcoholic stimulants, and upon the most advantageous mode of applying external heat. At the outset of the epidemic I gave brandy and whisky freely, but these remedies, if worthy of that name, were soon discarded. They invariably increased the irritability of the stomach, and when the patients to whom they had been administered recovered, a secondary fever, depending evidently on gastro-intestinal irritation and biliary derangement, protracted, in most instances, the period of convalescence. I cannot say, however, that either of these results followed upon the internal exhibition of carbonate of ammonia, and from this I infer that it is a less irritating stimulant than those I have been condemning. Independently of the bad effects which experience showed me to result from these, I was led to question their admissibility even upon theoretical grounds, and, in this respect, theory and experience seem to be completely in accordance with each other.

It has been determined that the quantity of carbonic acid exhaled from the lungs of a cholera patient is much below the usual standard, and that the blood is in a highly-carbonised condition, and is with difficulty made to imbibe oxygen. What are the effects resulting from the use of alcoholic drinks? Why, that the carbonic acid given off in the pulmonary organs is diminished in quantity, and that the arterial blood becomes of a darker hue than natural,—becomes in fact carbonised. Are we not justified, therefore, in concluding that the use of agents having such an effect is as unsound in theory as we have found it to be bad in practice.

With respect to the best mode of applying external warmth. At first I was prepossessed in favour of using it in a moist form, believing that the fluid might be absorbed by the cutaneous vessels, and the loss of the aqueous portion of the *liquor sanguinis*



taking place by the bowels in this way resupplied; but it never appeared to me to answer this intention. I observed that the plan of enveloping my patients in a blanket wrung out of warm water was very disagreeable to their feelings, as evidenced by the efforts which they made to throw the coverings off, and to instinctively, as it appeared, expose as much of the integumentary surface as possible to the atmospheric air. Now, we know that the skin is an important medium for aerating the blood, and we also know that the natural instincts frequently come into play, particularly in a state of disease. An observing physician should therefore never despise their dictates, and when he sees individuals, as in this instance, with their blood in a highly-carbonised condition, seeking to expose it to the action of the atmosphere, he should, instead of hermetically wrapping them up in wet blankets, rather strive to favour the admission of air to this fluid through every available channel, taking care, however, to supply it sufficiently warm so as to answer a double object.

With regard to what may be called the sequelæ of the disease; as these were not always the same, the treatment was of course varied to suit the pathological condition which the symptoms appeared to indicate. When the urine continued to be suppressed, blood was drawn by cupping from the lumbar region, blisters were applied there, and diuretics administered, particularly such as have been considered to possess the power of increasing the solid constituents of this secretion, as, for example, colchicum. I am not sure, however, that the latter, in any case, had this effect. The urine certainly sometimes re-appeared *post hoc*. But it occasionally did so without the use of any such agent, and it would therefore be assuming too much to assert that it was *propter hoc*.

In the four cases formerly mentioned, where spasms of a severe character came on during convalescence, they yielded as if by magic to a full bleeding from the arm and a calomel purgative. In these cases alone, there remains no doubt upon my mind of the treatment pursued having answered its object.

14. *Prevention*.—From the preceding remarks, it would appear that this formidable disease, when once fairly established, is little amenable to treatment. It therefore becomes a question, since we cannot boast of curing it, are there any means capable of preventing it? I believe that there are, but they are of such an extensive nature, that they can only be enforced by government or large public combinations.

In what follows, I will merely comment upon the preventive measures pursued in my district. Circulars were issued, advising the inhabitants to avoid those irregularities in diet, drink, &c., which were known to give a predisposition to the disease. But as these advices could not be enforced, in the most necessitous

cases, even read, I suspect that the printer was the chief gainer by them.

The next proceeding was to open a house of refuge for the reception of healthy individuals living in infected houses or lands. This promised to be a useful measure; but, unfortunately, the prejudices of the people were so great, that no amount of persuasion could induce them to enter it. True it is, that, during the course of the epidemic, the medical superintendents were invested, by the Central Board of Health, with the power of removing paupers by force if necessary. When, however, the poor came to know of this, they used every art to conceal cases from our knowledge, refused to avail themselves of our proffered aid, and, in fine, acted in such a way as to remove them, in my opinion, from the category of paupers. To have exercised my power under such circumstances would have been a very hazardous legal experiment, and I did not, therefore, think of trying it. Even when the parties were undoubtedly paupers, and voluntarily sought my assistance, I found that my usefulness as a practitioner was reduced to zero whenever I alluded in the most distant manner to the great power in my hands. The conclusion to which this led me was, that the duties of medically treating patients, and at the same time of using coercive measures towards them, were totally incompatible with each other.

The next plan of prevention was what was called cleansing, *i. e.* lime-washing the walls of houses in cholera localities. Cleansing operations of this kind were carried on in the village of Springbank on a tolerably extensive scale, but they did not appear to have any marked effect in checking the progress of the disease. It is certainly the case, that, in a few instances, where I contrived to get a cholera house emptied, lime-washed, and fumigated, the former inmates, on re-admission, did not suffer; but, in other instances, where the houses were merely vacated for a time, without any lime-washing or fumigation, the same thing occurred. Further, the disease appeared in many houses for the *first time* after cleansing, and it avoided altogether many of the dirtiest houses in the district,—for example, those in the corner indicated in the map by an asterisk. I would not have it inferred from this that I am an advocate for dirt, but only that I am of opinion that it is productive of very little injury when *confined* to the *walls* of houses. Had the bed, the body-clothes, and the skins of the parties been more frequently washed than they were with simple soap and water, then I believe that the result would have been fully more flattering.

The last, and, in my opinion, the most important step taken for the purpose of arresting the ravages of the disease, was the system of house to house visitation, instituted about the end of

December by Dr Sutherland. In my district, this led to the most beneficial effects. Two advanced medical students, selected by myself, devoted their whole time to visiting the houses in the infected and other localities. They were instructed by me to use no coercive measures, but to confine themselves, on all occasions, to simple arts of persuasion. This they did; and when the real objects of their visits became known, the inhabitants admitted them willingly into their houses. During the course of their visitation, they discovered immense numbers of cases of diarrhoeal affections. For the treatment of these, they carried appropriate medicines, which they administered on the spot. In several instances, they ascertained that members of the families they were visiting had a looseness of the bowels, although they were able to be out attending to their usual avocations. For these they left simple but efficacious remedies, such as chalk with opium, &c.

Of all the diarrhoeal cases thus *discovered*, one only of a simple, and three of a serous character, went on into cholera in spite of treatment.

I have, in another part of this report, directed attention to the fact, that many cases of undoubted malignant cholera were heralded in by premonitory diarrhoea, more especially towards the latter period of the epidemic. Can any one entertain a reasonable doubt of the possibility of checking the disease in this stage? I scarcely think so. My own experience leads me to speak very decidedly upon the point; and it therefore appears quite logical to conclude that many of the cases of purging, particularly those of a serous kind, discovered by my visitors, would have lapsed into the worst form of the disease, had they not been checked, as it were in the bud, by treatment. The number of cases of true cholera thus prevented, and the lives saved, cannot be even approximately guessed at, but it must have been considerable. With facts of this nature staring them in the face, it appears to me passing strange that individuals should be found, whether contagionist, non-contagionist, or contingent-contagionist, to declaim against a system which has practically proved so efficacious.

15. *Hospitals*.—This report would, in my opinion, be very incomplete did it not contain some allusion to the subject of hospitals. My views in regard to these are embodied in a letter which I addressed to Dr Sutherland on the 8th of March last; and as they have undergone no material alteration since writing it, I will confine myself on the present occasion to a general summary of its contents.

Table No. 1 shows, that during the months of November and December there were 474 cases of choleraic diseases, of which 82 proved fatal, being in the proportion of 17·3 per cent.; but if

the cases in class No. 1 be excluded, the mortality of the other two combined will be found as high as 51·3 per cent. During the two months specified, 24 patients labouring under different forms of the disease were sent to Clyde Street hospital, which is in the heart of the city, nearly two miles distant from my district, but at that period the most convenient to it. Of these, 15 died, being at the rate of 62·5 per cent. A question arises here as to the cause of this increased mortality. Were the cases sent to hospital of a more malignant type than those treated at home? or was the treatment adopted in the one instance less effective than in the other? My own impression is, that the cases sent away were mild in comparison with those allowed to remain in their own houses, and it was in consequence of such an impression that I gave a reluctant assent to their removal. As regards treatment, I have reason to believe that it was not only more active, but better superintended in the hospital than in the district practice. The true explanation of the problem, in my opinion, is, that the hospital was at too great a distance, and that the exposure and joltings to which the patients were necessarily exposed during the transfer increased their danger and contributed to the results referred to. This explanation derives weight from the fact, that several who left their houses with a moderately good pulse and a moderately warm surface of body reached the hospital in a state of profound collapse, from which they never rallied. It is strengthened in a still more striking manner by what I observed in the small district hospital at Woodside. Of the 96 cases treated there, 32 belonged to the immediate neighbourhood in the city parish, whilst a great number of the remaining 64 were brought from a considerable distance in the adjoining Barony parish. The mortality in the first instance was in the centesimal proportion of 37·5, whilst in the second it rose as high as 46·9. This increase, as I find by a reference to the hospital books, depended on the greater mortality occurring among those patients who came from the greatest distance. These results serve to confirm the opinion which I previously entertained, that in the event of cholera making its appearance in Glasgow, one or even two hospitals, however large and centrally situated, would be found quite inadequate for the requirements of such a rapidly fatal disease,—a disease, as has been well remarked, which commences where many others usually end,—a disease in which time is everything, and for the treatment of which consequently minutes are as precious as hours, or even days, in most other diseases. To meet it efficiently I advocated the necessity of having several small district hospitals, situated as near as possible to the infected localities, and into which those patients who could not be satisfactorily treated at home should be cautiously removed with as

little delay as possible. I am fully convinced that if this suggestion had been acted on from the beginning, a great saving of life and unnecessary suffering would have been the result.

A very superficial consideration of the facts narrated cannot fail, I think, to satisfy the most sceptical of the validity of my conclusions. In a pecuniary point of view also, I submit, that the adoption of the plan suggested would have been the wiser policy; for, admitting the above conclusion to be correct, and bearing in mind that the greatest liability to an attack of cholera is between the ages of 30 and 50, those ages at which parties are most likely to have large and young families,—it follows as a necessary consequence, that an ultimate saving to the parish would have been effected. This is, however, a question with which a medical practitioner has little to do. His primary object is to alleviate human suffering and to save life; and although the life be that of a pauper, he can see no difference between it and that of a peer, and will not willingly admit of its sacrifice to a principle of economy, which is as foreign to his mission as it is repugnant to the dictates of religion and humanity.

Many entertain the opinion, that patients affected with cholera should *never* be removed from their own dwelling-places under any circumstances; but those who think so cannot possibly know any thing of the kind of dens which many of the poor inhabit, and the neglect which the diseased amongst them often experience on the part of their attendants, when they have any. The parochial surgeon alone is fully cognizant of their condition in these respects. Often has he literally to grope his way to the miserable pallet of straw on which his patient is laid, and to lament the difference manifested not only by the immediate neighbours, but by the actual indwellers of the same wretched abode. In one melancholy instance I found an infant dragging at the breast of its dead mother; and in another my assistant discovered a woman seated dead upon a chair. Both had been deserted by their heartless neighbours and other attendants from a dread of infection, and both had been left to die without a friendly hand being by to administer to them even a draught of water during the last struggles of expiring nature.

I contend that there is an absolute necessity for having hospitals near at hand for the reception of patients so destitute as these, and that, however objectionable they may in some respects be, they are preferable to the complete abandonment of the wretched beings so circumstanced.

*Glasgow, May 18, 1849.*

ART. IV.—*On the Comparative Therapeutic Powers of Quinine and Bebeerine.* By THOMAS STRATTON, M. D. Edin.; Surgeon, R. N.; Member of the Edinburgh R. I. Clinical Society.

HAVING occasion to proceed from Britain to Canada, I sailed from London in the passenger-ship *Laurel* on the 7th of April 1849, and arrived at Quebec on the 30th of May. There were on board a number of emigrants (261 persons in all), and I acted as surgeon during the voyage. When ordering the supply of medicines in London, I was careful to include the sulphate of bebeerine, being desirous of using a medicine first introduced by Dr Rodie, and of the successful use of which, cases have since been published by Dr Douglas Maclagan, Dr Philip Maclagan, and others, and of which Professor Christison has given a favourable opinion in his standard work (*Dispensatory*, 1848). The specimen of bebeerine employed was from the manufactory of Messrs Innerarity and Co. of Glasgow. During the passage, it was given in the following eight cases, eight grains being dissolved in an ounce of water; but the most palatable mode of exhibiting it is with orange syrup.

*Case 1.* Captain K., aged 33, is troubled with dyspepsia and consequent flatulence and headach. A grain of bebeerine, three times a-day, for ten days, induced a decided improvement.

*Case 2.* Mr Henry W., aged 27, in debility after rheumatism. Two grains, three times a-day, for a fortnight, were given with increased appetite and strength.

*Case 3.* Henry Pearce, aged 19 months, is a scrofulous child, and has had several attacks of ophthalmia. During the voyage, he had scrofulous conjunctivitis, and a grain of bebeerine, three times a-day, was of evident advantage as a constitutional adjunct.

*Case 4.* Timothy Standing, aged 3 years, is a delicate child, and is now ill of infantile remittent fever.

April 15. For the last four days has had fever at night and in the forenoon, and is cool in the afternoon. To have three grains of mercurial chalk three times a-day.

April 17, 18. A grain of bebeerine each day at noon.

April 19. Was cool all to-day, and hot in the night only.

April 20. Was cool all day, and also in the night.

April 25. No return of fever; has had a grain of bebeerine daily since the 17th; the first dose produced vomiting.

Among the emigrants were a number of agricultural labourers from the marshy parts of Kent and Essex, and among them the four following cases of ague occurred. The sleeping-berths were close to the ship's side. During some days of rough weather,

the vessel's rolling caused her to leak a little, and hence most of the beds were rendered damp, in which condition they had to remain for some days.

*Case 5.* Martha Tucker, aged 3 years. She resided near Romney Marsh, in Kent. Her first attack of ague was one of quotidian, lasting for a week,—from the 18th to the 25th March.

April 15. Has had quotidian for the last four days. Applied to-day for the first time. The fit begins at 8 A. M. She is cold, without shivering or shaking, for an hour and a-half; is then hot, and afterwards perspires, and the paroxysm is over by about 3 P. M. Mercurial chalk six grains. Half a grain of bebeerine every evening and morning.

April 16. Had a fit to-day. 17th. No fit to-day. 18th. A grain of bebeerine twice a-day. No lip-eruption. A vesicular eruption on the nostrils.

April 19. No fit yesterday; to-day, no cold stage, but a hot stage in the afternoon.

April 20. No hot stage in the afternoon, but hot in the night.

April 24. No return of the fit. Nostril eruption has disappeared. She has taken fourteen grains of bebeerine.

*Case 6.* Elizabeth Tucker, aged 14 months. May 2. For the last three days has had quotidian, the first stage being coldness only, and commencing at 9 A. M. Mercurial chalk and rhubarb. Half a grain of bebeerine early every morning.

May 5. The fit returns daily, a little later every day. Two grains of bebeerine early every morning.

May 6. Had a fit to-day. May 7. No fit to-day. May 10. No return of the fit. No eruption on the lips or nostrils. Has been taking two grains of bebeerine daily for the last six mornings. Took six grains of it before, and eight after the ague was checked.

*Case 7.* James King, aged 20, farm-labourer, six years ago had ague in Essex. May 8. His bed is damp. The cold stage of ague began at half-past three P. M., and continued an hour and a-half. He was then hot, and afterwards perspired freely.

May 9. The fit began half an hour earlier. He applied for the first time. Calomel and compound powder of jalap.

May 10. Bowels open. He took eight grains of bebeerine at 11 A. M. At 4 P. M. the fit returned, but it was later, milder, and shorter than before.

May 11. Bowels open. At 10 A. M. he took twelve grains of bebeerine. At 4 P. M. the paroxysm returned.

May 12. At 11 A. M. took sixteen grains of bebeerine. The fit returned as usual. His bed, unavoidably, continues damp. No unpleasant cerebral symptoms after the dose of bebeerine.

May 13. At 10 A. M. had eight grains of quinine, and a fit in the afternoon.

May 14. In the morning had eight grains of quinine, and escaped the fit.

May 15. A vesicular eruption at the right angle of the mouth. Eight grains of quinine. No fit to-day. No unpleasant head or abdominal symptoms after the dose of quinine. He has the ague-complexion. His bed is still unavoidably a little damp, but not so much so as when he was taking bebeerine. Four grains of quinine.

May 17. Four grains of quinine.

May 19. No return of ague. Appetite continues to improve daily. Has taken thirty-six grains of bebeerine without effect; but, as his bed was damp, the medicine had not a fair trial in this case. He took sixteen grains before, and eight after the ague was checked.

*Case 8.* Robert Walker, aged 37, farm-labourer, lived near Romney Marsh in Kent, and had ague a year ago.

May 17. Has been exposed to cold and wet, and his bed is damp. Applied for the first time. For the last three nights had ague, the fit beginning at 7 p. m. To have calomel and compound jalap-powder. At 5 p. m. he had twelve grains of bebeerine. In the evening the fit returned, but it was two hours later in its accession, milder in its symptoms, and shorter in its duration.

May 18. At 5 p. m. took eighteen grains of bebeerine. Escaped the fit. Had no unpleasant cerebral or abdominal symptoms after the above dose.

May 19. At 5 p. m. had eight grains of bebeerine.

May 20. Very little appetite. No lip-eruption. Eight grains of bebeerine.

May 21. Four grains of bebeerine. No return of ague. He took thirty grains of bebeerine before, and twenty grains after the ague was checked.

*Remarks.*—Bebeerine and quinine may be contrasted as regards their medicinal effects and their price. In some individuals, unpleasant head symptoms are induced by the exhibition of quinine, and, in these persons, bebeerine is to be preferred. The price of bebeerine is five or six shillings an ounce, while quinine, at present, costs three times this sum; so that, even if the former had to be given in a dose three times larger than the latter, it would not be more expensive. But it would be doing injustice to bebeerine, to say that it required twenty-four grains to produce an anti-periodic effect equal to that of eight grains of quinine. Perhaps twelve grains of bebeerine, or from twelve to sixteen grains, may be considered as of equivalent efficacy to eight grains of quinine.

The following is a convenient mode of exhibiting bebeerine.



Thirty-two grains of the sulphate of bebeerine with fifteen drops of diluted sulphuric acid, one ounce of orange-syrup, and three ounces of water,—being a grain to a drachm or a tea-spoonful of the mixture.

*Montreal, Canada, June 1849.*

ART. V.—*On the Ganglia and Nerves of the Heart.* By ROBERT LEE, M. D., F. R. S., Fellow of the Royal College of Physicians, London. [From Philosophical Transactions of the Royal Society. 1849.]

HALLER, Wrisberg, Soemmering, and other eminent anatomists prior to Scarpa, have affirmed that no nerves are distributed to the muscular substance of the heart, and that its contractions do not depend upon nervous influence.

B. J. Behrends, a pupil of Soemmering, in 1792 published a memoir, entitled “*Dissertatio qua demonstratur Cor Nervis carere*,” in which it is admitted that nerves accompany the coronary arteries, and it is distinctly asserted that the muscular structure is entirely destitute of nerves.\*

The elaborate and splendid work of Scarpa, “*Tabulæ Neurologicæ*,” fol. 1794, has for its chief object the refutation of these erroneous views; but before referring to the discoveries of that great authority, I may proceed to state, that, in the magnificent plates of Mr Swan, only a few small branches of nerves which accompany the trunks of the coronary arteries have been figured, and the muscular substance of the heart is represented as almost completely destitute of nerves.

M. Chassaignac, who translated in 1838 Mr Swan’s “*Demonstration of the Nerves of the Human Body*,” has repeatedly denied, in the most positive manner, that any nerves except those which accompany the coronary arteries have yet been demonstrated in the heart. “*Anatomical research has hitherto ascertained in the heart only arterial nerves.*” “*The existence of nervous filaments, independent of the blood-vessels belonging to the fleshy substance of the heart, has yet to be demonstrated.*” P. 23.

Scarpa, however, had clearly delineated and described such nerves, viz. running on the heart independently of, and distinct from, the coronary arteries. In the work above cited, he has given five views of the nerves of the human heart, in some of which, *e. g.* Tab. IV., upwards of twenty filaments may be counted on the

\* *Ac primo quidem nervorum cordis examini scrupulosius intendens, tum observando, tum analogice concludendo didici nullos omnino nervos ne sureculum quidem in ipsam cordis carnem dispergi.*

same transverse line near the base of the heart, together with numerous anastomotic angular enlargements, which Scarpa does not specify as ganglions in his text. In the hearts of the larger herbivorous mammals, however, Scarpa describes and delineates both ganglia and fusiform enlargements of the nerves, which he calls corpora olivaria, and these not only upon the nerves at the base of the heart, but upon those that are spread over the superficies of the ventricle. His words are—"Præcipue autem nervorum cardiacorum trunci ad basim cordis et inter majora vasa arteriosa intumescunt in vera et genuina ganglia; in Equo autem et Bove etiam in iis ramis cardiacorum qui per cordis superficiem reptant nonnulla corpora olivaria gignunt."\* In Tab. VII. fig. 1, he represents, and at p. 42 specifies, some of these enlargements: one, *e. g.* marked 7, as a "ganglioformis intumescencia;" a second, marked 30, as a "cardiaci sinistri ganglion insigne." Scarpa also describes and figures several nerves independent of, and not accompanying, the blood-vessels of the heart, and avails himself of the fact to refute the conclusions to which Belrends had arrived in the treatise above quoted.

The following are the facts relative to the nervous supply of the heart which I believe myself to have established by examination of the foetal heart, of the heart of a child at the age of six years, of the heart of an adult in a sound state, of the human heart hypertrophied, and of the heart of the ox, and which the preparations are preserved to demonstrate.

The drawing No. 1, entitled "The nerves of the heart of a child nine years of age," natural size, represents the preparation displaying the nerves distributed over the exterior of the left ventricle, which come off from the "plexus coronarius posticus" of Scarpa,† together with a few filaments from the "plexus coronarius anterior," Scarpa. It shows the ganglions which Scarpa has delineated below the letters *a* and *b* in his Tab. IV., and also the slight enlargement at point of confluence of three or more nerves, which Scarpa has likewise figured, as *e. g.* between the nerves numbered 58 and 59, and in several other parts of the cardiac nerves displayed in the Tab. IV. above cited. In the place of the long and narrow loop on the nerve which Scarpa figures between the two chief branches of the posterior coronary artery, my preparation shows, as in a drawing which I preserve, a slender fusiform enlargement. The preparation also demonstrates nerves extending beyond the points where they end in Scarpa's figure, as far as the apex of the heart; and a slight expansion and flattening is presented by some of these apical filaments of nerves, and nerves not coincident in their course with the arterial branches are also

\* Op. cit. p. 2.

† Tabulæ Neurologicæ, fol. 1794, Tab. IV. Nos. 45, 46, 47, 48, 60, and 61.

shown in the preparation, which have neither been described nor delineated by previous anatomists.

In the dissection of the sound heart of the adult, depicted in the drawing No. 2, entitled "The ganglia and nerves at the apex of the left ventricle of the sound human heart," the additional nerves at the apex of the left ventricle are more clearly shown, in which three slender fusiform enlargements are shown on nerves accompanying the apical branch of the posterior coronary artery: there is also a well-marked angular enlargement at the point of junction of four nerves near a neighbouring branch of the artery.

The preparation which most distinctly establishes the fact of fusiform enlargements of the cardiac nerves, is that represented in the drawing No. 3, entitled "The ganglia and nerves of the left ventricle of a heifer's heart and cardiac fascia:" in which it will be seen that some of these fusiform ganglionic enlargements of the cardiac nerves are nearly in the same position as that of the "ganglion insigne," described and figured by Scarpa in the heart of the horse, Tab. VII.

The ventricles and auricles of the human heart, and those of the larger quadrupeds, are covered with two distinct membranes. The first or exterior of these is the serous membrane, which lines the pericardium, and is reflected over the whole surface of the heart. This membrane is connected rather firmly by cellular tissue with another tunic, which has scarcely, if at all, been noticed by anatomists. This second membrane has a dense fibrous structure, is semitransparent, and resembles, in a striking manner, the aponeurotic expansions or fasciæ covering muscles in other parts of the body, and, like them, sends numerous fibres or processes between the muscular fasciculi, blood-vessels, nerves, and adipose substance of the heart, which it binds closely together. This aponeurotic expansion, investing both ventricles and auricles, may be appropriately termed, from its structure and function, the fibrous membrane, or *cardiac fascia*.

The drawings, which have been executed by Mr West with the greatest pains and attention to accuracy, will supply the need of special verbal description of the nervous filaments, their anastomotic enlargements, and fusiform swellings; and the series of my dissections shows that the nerves of the heart, which are distributed over its surface and throughout its walls to the lining membrane and *columnæ carneæ*, enlarge with the natural growth of the heart, before birth, and during childhood and youth, until the heart has attained its full size in the adult; that the nervous supply of the left ventricle is greater than that of the right; and that, when the walls of the auricles and ventricles are affected with hypertrophy, the ganglia and nerves of the heart are enlarged like those of the gravid uterus.

*Postscript* (received December 21, 1848—read January 11, 1849.)—Since the communication above referred to was presented to the Royal Society, I have made a very minute dissection in alcohol of the whole nervous system of the young heifer's heart. The distribution of the ganglia and nerves over the entire surface of the heart, and the relations of these structures to the blood-vessels and muscular substance, are far more fully displayed in these preparations than in any of my former dissections. On the anterior surface there are distinctly visible, to the naked eye, ninety ganglia or ganglionic enlargements on the nerves, which pass obliquely across the arteries and the muscular fibres of the ventricles, from their base to the apex. These ganglionic enlargements are observed on the nerves, not only where they are crossing the arteries, but where they are ramifying on the muscular substance without the blood-vessels.

On the posterior surface, the principal branches of the coronary arteries plunge into the muscular substance of the heart near the base; and many nerves, with ganglia, accompany them throughout the walls to the lining membrane and *columnæ carneæ*. From the sudden disappearance of the chief branches of the coronary arteries on the posterior surface, the nervous structure, distributed over a considerable portion of the left ventricle, is completely isolated from the blood-vessels; and on these, numerous ganglionic enlargements are likewise observed, but smaller in size than the chains of ganglia formed over the blood-vessels on the anterior surface of the heart. In the accompanying beautiful drawings,\* Mr West has depicted, with the greatest accuracy and minuteness, the whole nervous structures demonstrable in these preparations on the surface of the heart. But the ganglia and nerves, represented in these drawings, constitute only a small portion of the nervous system of the heart, numerous ganglia being formed in the walls of the heart which no artist can represent. It can be clearly demonstrated that every artery distributed throughout the walls of the uterus and heart, and every muscular fasciculus of these organs, is supplied with nerves upon which ganglia are formed.\*

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ART. VI.—*On the Investing Fibrous Membrane or Fascia of the Heart.* By ROBERT LEE, M. D., F. R. S., Fellow of the Royal College of Physicians, London; Physician to the British Lying-in Hospital. [Medical Gazette, Vol. xlv. p. 224.]

In 1845 several anatomists in this country asserted that the uterus has no ganglia, and only a few small filaments of nerves,

\* For the drawings, which represent faithfully and distinctly the objects described, readers are referred to the Philosophical Transactions.

like sewing threads, which they affirmed do not enlarge in the slightest degree during pregnancy ; and in support of this opinion, the heart was adduced as furnishing a striking example of a muscular organ acting without interruption from the beginning to the end of life, though very sparingly, or not at all, supplied with ganglia and nerves. None of these anatomists themselves had ever dissected the nerves of the heart, and the plates of Scarpa and of Swan furnished the only evidence they could adduce in support of their opinion, that the muscular substance of the heart, like that of the uterus, is almost entirely destitute of nerves.

In September 1846, being dissatisfied with this evidence, I resolved to appeal to nature, and proceeded to dissect the nerves of the heart immersed in alcohol, as I had done those of the uterus, with magnifying powers of six and twelve diameters. The investigation was carried on during two years ; and from the examinations which I have made of the nerves of the healthy and malformed fœtal heart,—of the hearts of birds,—of the heart of the child at the ages of six and nine years,—of the heart of the adult in the sound state,—of the human heart slightly and greatly hypertrophied,—and of the heart of the young and adult ox,—the following conclusions may be deduced :—

1. That the blood-vessels and the muscular structure of the auricles and ventricles of the heart are endowed with numerous ganglia and plexuses of nerves, which have not hitherto been described or represented in the works of anatomists.

2. That the nervous structures of the heart, which are distributed over its surface to the apex, and throughout its walls to the lining membrane and *columnæ carneæ*, enlarge with the natural growth of the heart before birth, during childhood and youth, until the heart has attained its full size in the adult.

3. That the ganglia and nerves of the heart enlarge like those of the gravid uterus, when the walls of the ventricles are affected with hypertrophy.

4. That the ganglia and nerves which supply the left auricle and ventricle in the natural state are more than double the size of the ganglia and nerves distributed to the right side of the heart.

This anatomical demonstration of the ganglia and nerves of the muscular substance of the heart completely overthrows the last remaining argument employed by those physiologists who still defend the doctrine of Haller, that the irritability and contractility of muscular fibre is independent of nervous influence. This demonstration further clearly indicates the source of the actions of the heart as an entire organ, and how its detached parts can continue to contract after its total separation from the body. It likewise furnishes a satisfactory explanation of many phenomena

observed in the progress and treatment of organic diseases of the heart.

In prosecuting this investigation into the nervous system of the heart, I found that the great difficulty of dissecting and displaying the cardiac ganglia and nerves did not arise so much from their extreme softness, from their close and intimate connection with the blood-vessels, or from the quantity of adipose matter in which they were imbedded, as from the presence of a dense fibrous membrane or fascia which was interposed between the serous membrane and the muscular coat, of whose existence as a distinct tunic of the heart I had no suspicion, when these researches were commenced. In the most recent systematic writers on anatomy, the heart was represented as consisting of muscular and tendinous structures, blood-vessels, nerves, and absorbents, enclosed between two serous membranes.

The serous layer of the pericardium, which is reflected over the surface of the auricles and ventricles of the heart, is an extremely thin, smooth, and transparent membrane, which is torn by the application of the slightest violence, after it has been separated from the fibrous membrane or fascia, which is situated between it and the muscular substance of the heart. If an incision be made through both these membranes over the left ventricle, it is not difficult with a pair of fine forceps and a needle to destroy the cellular tissue by which they are united, and to demonstrate the existence of a serous layer and fibrous membrane over the whole surface of both auricles and ventricles. On examining the fibrous membrane when thus exposed, it is found to be possessed of great strength and firmness, glistening, semi-transparent, and resembling in all respects the aponeurotic expansions or fasciæ covering muscular organs in other parts of the body. It is much stronger over the ventricles than the auricles; and it adheres so firmly where it is in immediate contact with the muscular substance of the auricles and ventricles, that its separation often cannot be effected without tearing up some of the muscular fibres to which it is attached. From the inner surface of this fascia, which I have named the cardiac fascia, innumerable strong fibres pass to the blood-vessels, nerves, and muscular fasciculi and adipose matter. These strong slender fibres, connected with or proceeding from the inner surface of the fascia, accompany and surround all the blood-vessels and nerves, and they are interlaced together so as to form a peculiar stroma,—if it may be so termed,—of considerable thickness, between the fascia and all the various structures beneath, which it invests and binds together in the strongest possible manner. These fibres form a complete sheath around all the arteries, veins, and nerves, on the surface of the heart, and accompany them as they dip down between the

muscular fasciculi to which their branches are distributed throughout the entire walls of the heart, from the surface to the lining membrane.

From the preparations in the museum of St George's Hospital, and others in my possession, it is seen that the cardiac fascia exists in the hearts of the larger quadrupeds, in the human heart in the healthy and hypertrophied states, and in the heart of the child at the ages of six and nine years. It can likewise be demonstrated in the hearts of birds; and it is this fascia which chiefly gives to the foetal heart its remarkable firmness, when all the other muscular parts of the body are, as Mr Hunter observed, in a soft and almost gelatinous state. "The muscle which has the greatest resistance in an animal body to overcome," he observes, "is the heart, especially in quadrupeds; and this is perhaps the firmest in the body, being even firmer than those which have the above-mentioned resistance to overcome." "From the above account it must appear," adds Mr Hunter, "that muscles, in proportion as they are firm in texture, will be strong in action; it is at least demonstrable in the muscles of the same animal whose texture is different, and similar muscles in the male and female of the same species, and we may reasonably suppose that it will hold good in different species; and, therefore, when we find the muscles very firm in any one species, we may conclude that this species is stronger than any other species in which the muscles are tender and soft." "The heart of all partakes strongly of the two causes of firmness, and is perhaps the firmest muscle in the body."

The cardiac fascia is obviously one of the principal causes of the firmness and strength of the central organ of the circulation of the blood, as it binds together into one mass, and gives support to, the muscular fibres, like the fasciæ investing other muscles. The cardiac fascia is to the heart, I believe, what the external fibrous coat is to an artery, and it must have nearly the same effect in preventing dilatation and rupture of the ventricles during violent exertion. The thin feeble serous covering of the heart can possess little influence, and add nothing to the strength of the parietes; and probably, but for the fascia now described, the heart would often yield in all directions, especially at the apex. In a physiological point of view it therefore has appeared to me, that this fascia of the heart is one of its most important structures.

In a pathological point of view, the cardiac fascia is perhaps not less worthy of notice. Muscular structure, it is well known, is not liable to attacks either of common or of specific inflammation. It is impossible to avoid suspecting, that rheumatic inflammation of the heart has for its principal seat this dense fibrous

membrane lying between the serous and muscular coats of the heart, and that attacks of rheumatism of the heart do not commence primarily in the muscular structure. The *tunica sclerotica* of the eye sometimes becomes inflamed, softens, and yields, and from these changes it is known that sclerotic staphyloma, and other diseases, are the results. Whether in dilatation of the heart a similar morbid change is not first set up in the fascia, and what influence this fibrous membrane has in modifying all the diseases of the heart, future observations must determine.

The appearances of the cardiac fascia in the human heart, and in the heart of the ox, have been beautifully and most faithfully represented in the engravings which illustrate my paper "On the Ganglia and Nerves of the Heart," very recently published in the Philosophical Transactions, Part 1, 1849.

4 Saville Row, August 3, 1849.

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ART. VII.—*Clinical Contributions to the Pathology, Diagnosis, and Treatment of Certain Chronic Diseases of the Heart.* By CHARLES RITCHIE, M. D., late Senior Physician to the Royal Infirmary, Glasgow.

THE forms of cardiac disease to which I would refer in these contributions are,

*First*, One which is mainly a lesion in the capacity of the heart, and which has its initiative in impeded circulation of the capillary subdivisions of the pulmonary artery and veins. It consists, in its earlier stage, in a distended, and gradually in a dilated and permanently engorged state of the right heart, the persistent pressure of which, at its upper extremity, on the aorta, gives occasion eventually to enlargement of the ventricular walls, auricular opening, and also of all the cavities of the left heart; and, in the last stage, to increased congestion of the pulmonary, and dilatation of the systemic veins.

*Second*, Another form in which the cardiac lesions are primarily structural, and the point of departure of the hindrance to the circulation, is in either the aorta or the left heart; the first stage being made up by phenomena produced by the impeded flow of blood from the left ventricle or auricle, and the second stage by the addition of such as are dependent on regurgitation upon the left heart itself, the pulmonary, and, ultimately, the general venous system.

These affections unitedly compose from ten to twelve per cent. of the whole number of medical cases, exclusive of fevers, admitted into the Glasgow Infirmary.



The first stage of the first form is usually met with associated with bronchitis, with what is called asthma, and sometimes with tubercle; that of the second is often found to succeed rheumatism of a more or less marked kind, and frequently it appears as a complication of primary albuminuria and of phthisis. The advanced stage of both forms is constituted, along with an increase in the bulk, weight, and capacity of the heart, by great pulmonary and venous engorgement and by enlargement, and, subsequently, shrinking and induration of the liver, spleen, and kidneys, and by anasarca.

The external features of the different varieties of each affection maintain a general resemblance to one another throughout their whole course, but both are more distinct and typical in their first than in their latter stage. Although in this last, despite the accumulation and also the identity of pathological alterations, which exist, the distinctive lines of contrast between the two forms are still well and even strongly preserved.

The cases on which the following observations are based, amounted to 185, of which 72 belonged to the first class, and 113 to the last; and of these latter, 37 were aortic, 25 mitral, 16 aorto-mitral, 8 mitro-aortic; in 5, the aortic, mitral, and tricuspid valves were affected; and, in 1 each, the pulmonary and tricuspid valves exclusively. In 20, the precise locality of the alteration of structure was not recorded.

The objects which I propose in this and subsequent communications are to delineate the characters of the two affections respectively in their progression toward a fatal issue; to seek, by an extended comparison of the symptoms in each, to establish the diagnosis of the one from the other; and, lastly, to endeavour to place the principles and details of treatment in both on a simple and correct footing.

In pursuance of these views, I would proceed to treat the subject under the two general divisions of

I. Dextral, or Simple Congestive Disease of the Heart.

II. Sinistral, or Structural Disease of the Heart and Large Blood-vessels. And,

1. Of Simple Congestive, or Dextral Disease of the Heart.

This disordered condition varies in pathological character, in the intensity and number of its symptoms, and in the indications of treatment which it presents, according as it is met with in its initiatory, its transition, or its closing stage, or as it is complicated with lesions of other organs. I would, therefore, consider it in

A, Its earlier stage.

B, Its transition period.

C, Its conclusion.

D, Its complications.

And first of the disease in

A, Its earlier stage.

This initiatory period has no definite chronological, or, indeed, any very precise pathological limits. It is constituted by varying degrees of chronic pulmonary congestion, bronchitis, and emphysema of the lungs, and by a state of distensibility, and, at length, of moderate permanent dilatation of the right heart; and its duration ranges from a couple of years upwards, according to the exposure of the patient or not to renewed bronchitic attacks, or other causes fitted to arrest the circulation of the pulmonic capillaries.

I am not prepared to pronounce on the precise mechanism of this obstruction; but the general aspect of the affection appears to warrant the belief that, as in sinistral disease of the heart, the symptoms flow out of a mechanical impediment to the systemic circulation; so in this, that there is an anatomical obstruction of some kind, established probably in a great portion of the extended net-work of vessels which encircle the pulmonary cells, to the progression of the pulmonic blood.

I subjoin some illustrations of the initiatory stage of this affection.

Case 1.—*Neglected bronchitis succeeded by increased congestion of the pulmonary circulation. Treatment of a moderately active kind not well borne, and case managed successfully by expectorant and diuretic medicine.*

March 20, 1848. — M'Gregor, aged 42, a tailor. About three months ago, this person, after exposure to cold and wet, was seized with severe cough and slight expectoration. This state continued for several weeks, when he had a violent attack of dyspnoea, renewals of which, much increased on the most ordinary movements, and accompanied by aggravation of the cough, and by tough frothy expectoration, have appeared at short intervals till the present time. He complains much of debility, and of pain across the breast when the cough is protracted. Loud sonorous rhonchi heard over thorax. Percussion and heart sounds normal. Pulse 104; tongue dry, and covered with white fur.

Potassio-tartrate of antimony, twelve grains; cinnamon-water, six ounces. Dissolve. Two, or should the medicine be tolerated, three drachms of this solution to be given every second hour. Calomel, twelve grains; powder of opium, four grains. Form into twelve pills; one to be given every four hours.

23d, Omit the solution and pills. To have ten grains of nitre and one grain of ipecacuan every four hours.

27th, To have twenty-five grains of ipecacuan as an emetic.

April 3d, Dismissed cured.

It will be observed, that, as is the nearly uniform fact in this affection, the case now quoted occurred in the winter months. Of 41 examples to which I referred on this point, 11 happened in December, 8 in January, 5 each in November and October, 4 in April, 3 each in February and March, 1 each in May and September, and none in June, July, or August.

The patient was a tailor. The seclusion in impure warm air of individuals in this employment, predisposes highly to bronchial inflammation, and their position while at work often directly excites dilatation of the pulmonary cells and impeded circulation of the right heart. Shoemakers, and especially miners who work in narrow seams, and are exposed to the inhalation of coal dust, are often similarly affected from the same causes.

In dextral cardiac disease, the bronchitis is always a primary and almost always a prominent pathological condition. At the early stage, at present under review, the inspirations rarely exceed from 20 to 24 in number per minute, and, in the slighter forms, the respiration shows little more than some exaggeration of the ordinary murmur in front, and moist crepitation among the tubes behind. In graver cases, the breathing becomes bronchial in the latter situation and under the clavicles, and the respiration is accompanied by a snore or a wheeze. As the malady acquires standing the bronchi get loaded with sputum; moist rhonchi become copious throughout the whole of the lungs. These convey the idea to the observer at once of their being remarkably superficial, and also pervading.

The expectoration in the earliest stage is generally scanty; sometimes it is more copious, but thin and salival, or at most frothy; and it is only as the case proceeds and the smaller tubes get involved in the inflammation, that the sputum becomes glutinous, dense, ropy, and contains minute bullæ. At a more advanced period still, the various changes usually observed in the colour, quantity, and consistence of bronchitic sputum take place.

The cough in simple congestive cardiac disease is usually moist, paroxysmal, and often continuous during night. The dyspnoea also is generally moist, and in bad cases, even at this early stage, there may be some crowing, with sense of constriction at the glottis and at the sternum. In the man whose case has been narrated, the dyspnoea had a peculiarity which is commonly limited to affections of the left heart. It was aggravated by the slightest movements of his body. In reference to his cough, expectoration, and the stethoscopic signs, they were such as might be supposed to be present when the larger bronchia are the chief seat of the disease.

This case I quote as an example of the way in which a groundwork is laid for the superinduction of dilatation of the air-cells. Thickening and softening of the bronchial lining, with diminished

elasticity of the pulmonary vessels and cells, soon become permanent conditions in persons so situated. In the following cases, such changes appear to have been consummated.

Case 2.—*Bronchitis of four months' standing, with emphysema of the lungs and certain persistent lesions of the respiration.*

3d Dec. 1847, — Corrie, aged 27, a labourer. Four months ago, during convalescence from fever, he became affected with cough, and five weeks ago, his increasing weakness compelled him to desist from labour, and, two weeks since, to take to bed. Pulse 92, rather soft. Tongue moist, red, and covered by a yellow fur. Cough, dyspnoea, and expectoration troublesome. Respiration sibilant, under clavicles gurgling, but most generally it is sonorous or strongly crackling over lungs. Bowels regular.

He had one grain of calomel and about one-third of a grain of opium every six hours ; and on the 8th he was ordered an emetic of ipecacuan.

10th, Much relief of general symptoms. Pulse 92. Sense of constriction in breast.

Blister to sternum. Potassio-tartrate of antimony, twelve grains ; water, six ounces ; tincture of opium, one scruple. Two drachms to be taken every two hours.

13th, Does not tolerate the solution. Repeat the blister.

23d, Pulse 72. Average expiration only 125 cubic inches. Spits about an ounce of thin, slightly opaque sputum in the day. Cough is troublesome at night. Increased sonorousness of thoracic walls ; soft, double bronchial breathing throughout lungs, with here and there moist subcrepitous rales.

27th, Dismissed relieved.

Most of the conditions here reported may be regarded as being to some extent fixed in their nature, and also as interfering at once with the healthy circulation of the lungs, and maintaining a predisposition to renewed acute attacks.

In another case (Hugh M'Lauchlin) of nine months' standing, the thorax is described as being " prominent and highly resonant on percussion," or, according to a second account of the same case on a subsequent admission, " the thorax is bulged out considerably all over, but most so in the right antero-superior region ; the sternum is arched ; the natural dulness over the heart is replaced by sonorousness ; the respiratory murmur is feebly heard all over the lungs ; the trapezius is large and fleshy ; and the efforts made at respiration are out of all proportion to the murmur heard. There is lengthening of the second sound of the heart, epigastric tenderness and pulsation, the latter being specially noticeable during inspiration, and the normal impulse of the heart near the left mammilla has disappeared. His expiration amounts only to 125 cubic inches, and at the most extreme point of expiratory effort of

which he is capable, the cardiac region remains sonorous, and the circumference of the thorax scarcely affected. There is only slight dilatation of the jugular veins."

In this latter case, and that marked No. 2, the principal additional features are those which refer to the emphysema of the lungs, and to the altered sounds of the heart.

Under the first of these heads are the double breathing from increased irritability of the air-cells; the augmented sonorousness of the thoracic walls, commencing, as is generally the case, in front, and marking the diminished power of expiration; the extension of this resonance to the normally dull space around the cartilage of the left rib, the consequence of the protrusion of the fifth inflated lung in front of the heart; the hypertrophy of the auxiliary muscles of respiration, as the trapezius, proving the habitual inaction of the intercostal muscles and the permanency of the dyspnoea; and, lastly, the remarkable reduction in the quantity of air expelled by the lungs, the amount being diminished in both patients from 240 cubic inches at each expiration, which is the standard quantity, to so small a volume as 125 cubic inches.

In reference to the second head mentioned, or that of the altered relations of the circulatory system in those patients, these consist in the removal of the heart's impulse from the vicinity of the nipple to the epigastrium, which results from the base and right side of the organ being thrust down on the diaphragm by the expansion of the lungs; also of some distension of the jugular veins, one of the first effects of some temporary arrest of the pulmonary circulation; and, lastly, the lengthening of the second sound of the heart.

In regard to the extension of the diastolic sound of the heart, which is here mentioned, I have been accustomed to esteem it, when heard early, as in this case, as being strictly dextral in its origin, and as a mark of hindrance either of a vital or of a mechanical kind to the free progression of the right systolic wave, and also as at once a proof and a measure of the regurgitating strain, which, in the present class of cases, is set up on the valves of the pulmonary artery.

The observation of this symptom is most conveniently conducted by passing the stethoscope along a line stretching from the cartilage of the second left rib to the sternal side of the right mammilla, and from this along the right side of the sternum to the ensiform cartilage. The sign in question may be confounded with a prolongation of the second cardiac sound, which is sometimes heard at a more advanced period of this affection over the aorta, when the left heart is beginning to suffer from the dextral congestion; but in this latter event the symptom is found to extend into the arteries of the neck.

That reflux of blood into the right ventricle during the diastole of this chamber does actually occur at an early stage of chronic bronchitis, is apparent from a consideration of the sequence of phenomena to which that disease gives rise; and also from the actual observation of early cases. And, notwithstanding the apparent adaptation often of the pulmonary and tricuspid valves to their respective openings after death, it is rendered certain by the great dilatation of these openings in the same subjects, and by the increased size of the right heart itself, that the impediment presented to the course of the pulmonic wave during life, does occasion habitual reflux, even in cases where the valves appear perfect after death, and that also without the production throughout the illness of any other physical sign in this situation than the prolongation of the second cardiac sound.

The reality of this engorgement of the right heart in the early stage of the disease under present consideration, appears sometimes from the supervention of increased precordial dulness, sometimes from distension of the jugular veins, especially during expiration or coughing; and, at others, from the rose colour of the lips, the violet hue and congestion of the tongue and inner surface of the cheeks, and even from the distension occasionally of the ranular veins. I subjoin an example or two.

*Case 3.—Emphysema of some extent and standing; congestion of tongue.*

— More, aged 49, a coal porter, Jan. 11, 1847. About nine years ago this patient had a severe attack of some pulmonary inflammation. Almost ever since that time he has been subject to cough, aggravated in cold weather. For last seven weeks cough has been unusually severe, and has been attended by shortness of breath, with sensation of tightness in thorax. Strength has also fallen off. At present slight emphysematous crepitation, and occasional bronchitic rales, are heard over most of chest. Chest barrel-shaped, and percussion everywhere clear. Breathing tranquil; sputum consists of muco-purulent masses, mixed with others which are black and viscid. Pulse 88, good; heart's impulse most marked in epigastrium; sounds faint and obscured at apex; bowels rather slow; appetite impaired; does not complain of thirst; tongue moist, large, dark-coloured, and pretty clean. Never had œdema.

Powder of ipecacuan, six grains; nitrate of potash, one drachm; bitartrate of potash, two drachms. Mix, and form twelve powders; one to be taken every five hours.

14th, Simple syrup, mucilage of gum arabic, of each two ounces; tincture of hyoscyamus, two drachms; tincture of squill, one drachm. Mix; half an ounce to be taken every six hours.

16th, A pint of porter daily.

28th, Dismissed relieved.

Case 4.—*Bronchitis of six months' standing ; prolongation of second cardiac sound ; precordial dulness ; some distension of jugular veins.*

John M'Lachlan, aged 43, a mason. March 13, 1848. About six months ago this man was seized with a severe cough, accompanied by viscid expectoration. Had also some dyspnoea, increased on exertion, and during the night. The cough has continued till admission, but has become worse within the last fortnight, with copious, frothy sputa, occasional fits of dyspnoea, great sense of debility, and some pain, with the cough, across the chest ; a sonorous rale is well heard over thorax. There is greater dulness in the vicinity of the bottom of the sternum than natural, and some prolongation of the diastole of the heart. The jugular veins are somewhat turgid. Pulse 92, of good strength.

To be cupped at the epigastrium to four ounces. To have half a grain of calomel, and one sixth of a grain of opium every two hours ; one drachm of Epsom salts to be taken three times daily ; to have the foot bath every night.

14th, A blister to be applied over the sternum.

15th, To have twenty-five grains of ipecacuan as an emetic.

17th, To omit the calomel and opium, and take two ounces of infusion of columba three times a-day.

31st, Dismissed relieved.

In the following case death happened from an accidental intercurrent cause ; and the fact of an early stage of extensive engorgement of the right heart in this affection is shown to admit of positive proof. It is extracted from the journal of another of the physicians of the Royal Infirmary.

Case 5.—*Habitual bronchitis ; severe uterine hemorrhage after delivery ; death nine weeks afterwards from debility ; right ventricle of heart much dilated ; right auricle of enormous size ; tricuspid opening enlarged ; left heart healthy.*

Mrs Marshall, aged 44, from the country, 4th June 1833. This patient, about six weeks ago, after delivery, had severe and long-continued flooding, which has left her very much exhausted. Four weeks since legs and abdomen began to swell ; former are œdematous, and the latter affords a distinct feeling of fluctuation, though not tense. Face is also swelled, particularly below eyes. Thirst is urgent, but fluids swallowed become sour on stomach, and are frequently vomited. There is also much flatulence ; she complains of great nervous palpitation, action of heart being frequent, and accompanied by slight bellows sound. There is some cough, with clotted mucous expectoration ; but stethoscope gives no particular indication, except puerile respiration in front. Has been habitually asthmatic ; pulse 120 ; very feeble ; tongue clean ;

bowels slow ; urine scanty and high coloured. Nothing of moment occurred in the progress of the case ; she sunk gradually from loss of strength, and died on the 24th.

*Inspection.*—One ounce of serum at base of brain ; the usual quantity in the ventricles ; substance of brain pale and soft ; right ventricle of heart much dilated ; auricle of enormous size ; auriculo-ventricular opening larger than usual ; probably could not be closed by valve ; left side healthy ; about three ounces of yellow serum in pericardium ; left lung extensively adherent to pericardium ; three pounds of serum in right pleura ; lungs œdematous ; liver hard.

In another puerperal female admitted under my own care the appearances were pretty similar.

Case 6.—*Cough and expectoration of about six months' standing ; acute pulmonary attack from cold three weeks before admission, and some days only after parturition ; death on 4th day of residence from exhaustion ; right ventricle dilated ; lungs and bronchiæ congested.*

Margaret Burnet, aged 36, married. June 13, 1848. Three weeks ago she gave birth to a child, and four days after, in looking for lodgings, she was exposed to cold and wet, from which a slight cough and expectoration which she had had since the previous winter, became worse, accompanied by great dyspnœa and debility, all which continue. The sputum is bronchitic and copious. Extreme resonance of voice in right *suprascapular fossa* ; large rhonchi in corresponding portion of left thorax. Respiration rapid ; much sweating ; complexion anæmic ; action of heart natural ; pulse 96, feeble ; feet and legs slightly œdematous ; appetite good ; bowels open ; tongue clean.

To have the fourth part of a calomel and opium pill every four hours.\* Take of powder of ipecacuan, twelve grains ; nitrate of potash, one drachm ; mix, and divide into twelve papers, of which let her have one three times daily.

16th, To have four ounces of wine.

17th, Death at 9 A. M.

19th, Inspection.

Pericardium contained about eight ounces of fluid ; right side of heart dilated and covered slightly with fat ; left side of heart and its openings natural ; lower lobe of right lung adhered to diaphragm ; lining membrane of its larger bronchia much congested, which, as well as the smaller branches, were loaded with frothy mucus ; and in its lesser, which was very soft and friable, were a few scattered miliary tubercles ; left lung was much contracted at the middle of its lower lobe, on which was a white

\* The calomel and opium pill of the hospital consists of two grains of calomel and somewhat less than half a grain of opium in each.



patch three inches in diameter, puckered up with a central line, apparently the cicatrix of an old abscess. The other parts of this lung exhibited the same appearances as were observed in the right. Much thickening and strong adhesions of peritoneum around the uterus, which, as were also the small intestines, was highly congested. Several yellow bodies in ovaria.

The fact of those individuals being puerperal women is worthy of remark. There appears to be a proclivity to cardiac disease generally in the gravid state. Of 46 such female patients, of whom I took a note, 15 were puerperal. Twenty of these were examples of dextral or simple congestive disease; and of these, eight, or 40 per cent., were either gravid or recently delivered. The abdominal congestion, and, in particular, the portal congestion which prevails in pregnancy, has a marked retarding influence on the pulmonary circulation, which in sound lungs predisposes, and in others excites, to bronchial inflammation and dilatation of the right heart.

In the two cases last reported, an additional symptom to those yet noticed as constituting dextral cardiac disease is recorded. I allude to the dropsy. This lesion is an indication of an overloaded condition of the general venous system, which, in disease of the heart, is commonly associated with an impediment to the flow of blood by the left ventricle; and its presence in the cases I have here given might seem, therefore, to demand their removal to some division of this paper, of which sinistral obstruction was an element. The declared healthiness of the left side of the heart in both patients, however, is a sufficient reason for regarding them as proofs, that in certain circumstances, as when the bronchitis or pulmonary congestion is intense, dextral disease of the heart may occasion a systemic venous plethora and serous effusion, independent of any hindrance to the aortic circulation. I subjoin what seems to me a striking example of the same fact.

Case 7.—Mrs Brodie, aged 40, 29th January 1834. There is considerable general anasarca, and the belly is swollen, fluctuant, and painful on pressure, especially at the epigastrium. The face is of a leaden hue; the lips prominent and livid. She is unable to lie on left side, and has difficulty of breathing, and cough with expectoration of thick yellow mucus. The jugular veins are turgid, but do not pulsate. Chest generally sounds clear on percussion. Below the clavicles, vesicular respiration is not to be heard. There and lower down, both before and behind, loud sonorous rattles are heard; and in infero-posterior parts of both sides of thorax, there is subcrepitous rattle. No disease of heart detected by stethoscope. Pulse about 100, small and regular. Tongue furred, bowels slow, and urine scanty. Some appetite; sleeps little. Complaints came on five weeks ago with cough and

dyspnœa, after exposure to cold. The dropsical effusion was preceded by general pains. Has been subject to cough after the least exposure to cold for some years. No treatment before admission.

A common enema. A draught of one drachm of ammoniated tincture of opium, and half the quantity of sulphuric ether.

30th, Pulse 96; face livid. Take of acetate of potash, six drachms; sweet spirit of nitre, half an ounce; water, one pound. An ounce of the solution to be taken every second hour.

February 2d, Considerable purging. Face livid. Breathing oppressed. Pulse 84, feeble. Skin warm and moist. Had an anodyne clyster, which she still retains. Repeat the enema if required.

3d, Purging abated by the injection, which has not come off. Pulse 84, feeble. Breathing panting. Died about seven in the evening.

*Inspection.*—Lungs on opening the thorax did not collapse, but remained distended, completely filling their containing cavity and overlapping the heart. A great portion, particularly of the upper lobes of both, was emphysematous; and inferiorly and posteriorly there was some œdema of their structure. Some of the air-cells were capable of containing a cherry-stone. There was no effusion in either side of chest. The bronchial tubes were filled with viscid mucus, and their lining membrane was very vascular, of a deep red colour, and somewhat thickened and softened. Structure of heart normal, but its right cavities were gorged with coagulated blood. About fourteen ounces of straw-coloured serum were found in the peritonæum, which membrane presented a universal blush of red, but no lymph.

It remains that I should offer some remarks on the principles and details of treatment in the earlier stage of simple congestive disease of the heart. And, first, of the principles of treatment. These are, according to the symptoms present,

a, The removal of bronchial inflammation, or of congestion of the pulmonary capillaries.

b, The restoration of healthy tone to the tissues of the bronchial lining and weakened pulmonary cells.

c, The improvement of the general health.

In attempting to apply the first of these principles, or the removal of either the bronchitis or the pulmonary engorgement, there are various details.

*Blood-letting.*—Patients of any kind, and labouring under any disease, bear general bleeding ill in Glasgow; and, when used in the present affection, the remarkable exhaustion it occasions often prevents it being carried further than a few ounces. The employment of cupping between the shoulders, or at the epigastrium,

requires, in hospital cases here, little less precaution, although, in recent and severe seizures, or in milder attacks in the country, it may be resorted to with greater advantage than in patients with a similar amount of pulmonary embarrassment arising from disease in the left heart.

*Tartrate of Antimony.*—When taken alone, or along with calomel and opium, this salt generally rendered blood-letting, in the present kind of cases, unnecessary. The patients who required it were such as had frequency of pulse of recent occurrence. In these, more or less elasticity, if not hardness, of the radial pulse, and other signs of reaction, as floridity of the countenance, white tongue, thirst, and heat of skin, are present. The formula I employed was prepared with either two or four grains of the tartrate to the ounce of water, and the dose was made to depend altogether on the quantity which could be tolerated. It is borne better when preceded by a little food, or even by a morsel or two of biscuit; and, when it does not occasion vomiting, or diarrhœa, or aphthous inflammation of the throat, it may be used so long as the symptoms demand it.

*Mercury.*—The preparations of this which I trusted to at the early stage, were, in mild cases, either the blue powder or blue pill, given in doses of half a grain three times daily, conjunctly with a grain of ipecacuan; five grains each of nitre, sulphur, and cream of tartar, or some modification of these materials and quantities; and, in more severe and acute cases, I employed calomel in half or quarter grain doses every two or three or four hours, along with a minute proportion of opium, and sometimes with one or more of the adjuvants already mentioned. The mouth in dextral disease of the heart is quickly affected by mercury, and this also according to the advancement of the case. In order, therefore, to obtain its constitutional effect, it is needful to give it in fractional doses at short intervals, and to suit the degree of its division to the period of the complaint. When they are not prematurely affected, tumidity of the gums is commonly followed by a subsidence of the severity of the symptoms, the result, I presume, of the increased activity of the capillary circulation which the mercury excites.

*Derivants.*—These acted either on the bronchial surface, increasing the flow of expectoration, as mercury, ipecacuan, the inhalation of hot vapours, and emetics; or they excited the action of the liver, as the medicines just named, and sulphur, muriate of ammonia, rhubarb, senna, sarsaparilla, and dandelion; or they emptied the remote extremities of the portal vessels spread out in the intestinal tube, as saline purgatives; or they stimulated the kidneys, as supernitrate of potash, nitrate of potash, cream of tartar, squill, and digitalis; or they increased the quantity of

blood in the thoracic walls, as blisters and rubefacients ; or in the inferior extremities, as hot foot baths with or without mustard.

The selection of one of those means of relieving pulmonary congestion, or the combination of any number of them, would depend on ever-varying individual circumstances.

The inhalation of the vapour of hot water was thus indicated, conjointly with mercurials, ipecacuan, tartrate of antimony, digitalis, sinapisms to the thorax, antiphlogistic diet, and saline laxatives, when the sputum was scanty, and very albuminous, and the general reaction considerable ; emetics proved beneficial in an opposite state, either by stimulating a simply relaxed and weakened bronchial lining to pour out fluid, or as mere evacuants when the bronchia were already loaded with mucus ; blisters of cantharides answered in slighter cases or in the advanced progress of those which were more severe, while in more recent and purely phlogistic seizures, sinapisms applied for a few minutes to various parts of the thorax once or twice daily, suited better. The remedies fittest to act on the liver could often be advantageously joined with such as might produce watery stools or diuresis. Thus, Battley's or Squire's extract, or liquor of taraxacum, was used by me, especially in private cases, along with solution of the muriate of ammonia, or of the sulphate of magnesia. The supernitrate of potash, prepared by acidulating a saturated solution of sesquicarbonate of potash, with a slight excess of nitric acid, and taken every few hours in doses of a tablespoonful, diluted with water, is best used alone. Its action, when taken in smaller doses, is on the kidneys, and in larger on the bowels.

The expediency of such methods of relieving pulmonary plethora in the early stages of this affection is indicated by the fact, that in the later stages of it the same discharges are often set up naturally. Vicarious perspiration is an accompaniment sometimes of the advanced disease, as well as diabetes, and habitual catharsis, and has to some extent a similar remedial import. The propriety of early, long continued, warm foot baths, is suggested by another feature of the confirmed or completed malady. I refer to the circumstance, that in this stage, patients, when oppressed with dyspnoea, are almost always observed, besides using other contrivances to increase the capacity of the thorax, to throw their oedematous legs over the side of the bed. In this situation these act as a kind of cesspools to the fluids, and it is thus also that the protracted hot or warm foot bath, besides giving a fillip to the action of the heart, and consequently to that of the enfeebled pulmonary capillaries, proves serviceable by augmenting the amount of fluids actually in the limbs.

I have said that the second indication of treatment in the early stage of simple congestive disease of the heart is, *b*, The resto-

ration of healthy tone to the tissues of the bronchial lining and weakened pulmonary cells.

It is presumed that, after the subsidence of the acute symptoms, there remains a measure of retarded circulation, or at least of disposition to engorgement, in the remoter subdivisions of the pulmonary artery, and more or less habitual abnormal retention of air in the cells of the lungs. The treatment resorted to is therefore of a nature which may be thought fitted to remove such a condition. Of this kind are

*Emetics.*—The succussions of body which these occasion at once quicken the circulation of the vessels of the lungs, empty the distended air-cells of their air, and promote secretion from the bronchial membrane, in all which ways they are salutary, and should be repeated according to circumstances.

*Change of Air and the use of various Inhalations.*—These act as alteratives, stimulating the respiratory surface. Dry air, if mild and not highly rarefied, is very suitable at this early stage, although a marine atmosphere is often found to answer. Fumigations of tar-vapour, or the fumes, when ignited, of bibulous paper, saturated with nitre and dried, are the only forms of inhalation which I have used for this purpose, and the former with much advantage.

*Sudden Application of Cold to External Thorax.*—The efficacy of this in exciting the respiratory act in asphyxiated persons is well known. A stream of cold air directed on the surface, or the sudden sprinkling or dashing of cold water on the face, neck, spine, or breast of the new born child, or of persons in a swoon, or poisoned by prussic acid, opium, carbonic acid gas, &c., are often successful in inducing breathing. These are so many examples of an impression made on the sentient cutaneous nerves being reflected from the nervous centres back on the respiratory muscles; and it is perhaps from the operation of the same principle that the sudden and rapid application, in the present disease, of towels soaked in cold water, or vinegar and water, is calculated, when used for some time, to improve the condition of the bronchial membrane and air-cells. It requires to be executed with rapidity, in a warm apartment, and to be followed by smart frictions with hot flannels. The vascular reaction which it thus occasions on the skin is also advantageous.

*Well-regulated Exercise in a Pure Atmosphere.*—The objects to be kept in view in this department of the treatment are various; as to strengthen the muscles of respiration, to equalize the circulation, and to promote the arterialisation of the blood. The nature and amount of the physical effort recommended will therefore be such as may gently and steadily augment the force of the heart's action, and moderately accelerate the respiration, while

a healthy stimulation of the bronchial and cutaneous surfaces is at the same time provided for.

The concluding remedial indication which I have spoken of is, c, To improve the general health. This in the early cases which form the subject of the present communication, may safely be trusted to nearly the same general principles and details which guide us in the convalescence of other diseases. The various kinds of white animal food will, of course, be preferred at first, as being less stimulating than others; and if there be any specialty which requires notice, it is, that whether the diet consists of these, or of farinaceous substances, or partly of both, it should be as dry and solid as possible. The object is to nourish consistently with not over-stimulating, or increasing unduly the quantity of the blood which is in the circulation.

(To be continued.)

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ART. VIII.—On the *Makrocephali* of Kertsch in the Crimea.

By Dr H. RATHKE. (Zeitschrift für Physiologie von Tiedemann und Treviranus, B. V. H. I.)

AROUND Kertsch, the *Panticapæum* of Strabo, are seen, to the distance of several wersts, a countless assemblage of small grassy elevations, some of which are conical, others spherical in shape; and the smallest of which exceed the height of the human body, at least two or three times. These elevations, examination of very many of them have shown to be the burying places of Greek colonists, who in ancient times inhabited the eastern part of Crimea. Besides different utensils, small statues and limestone slabs, embellished partly with figures, partly with Greek inscriptions, there have been found in these burrows also remains of human skeletons, the skulls of which presented nothing remarkable. Between these burrows, however, and the level grounds, there have been several times found, without any trace of coffin-like neighbourhood, human skulls and fragments of skulls, which possess a figure deviating very much from the normal shape of the Caucasian race. These skulls, in short, present an unusually great elevation in proportion to their transverse diameter, and they consequently struck much those persons who had no more particular knowledge of the structure of the human body. On account of their elevation, these skulls were named at Kertsch by antiquarians, the skulls of *Makrocephali*.

An incomplete specimen of a skull of this kind, consisting of the larger part of the cranial cavity, I found in that town in the museum devoted to antiquities. According to the figures of it

which I sketched on the spot, and which are published in Tiedemann's Journal, it has a great resemblance to the corresponding part of a skull which is delineated in Blumenbach's *Decades Craniorum*, and to that which was sent from Russia by Baron von Asch, under the name of a *Makrocephalus*, but without more specific information of the place in which it was found. It is accordingly not improbable that this skull, acquired by Blumenbach, was found in the eastern part of the Crimea.

The fragment of skull seen by me at Kertsch must have belonged to a female, if we draw any conclusion from the slenderness of its individual parts. It possesses some resemblance to a truncated cone, and shows, at first glance, that it is unusually high in proportion to its transverse diameter, and that the separate bones of which it is composed are separately contorted. The distance of the centre of the frontal bone from the centre of the flat portion of the occipital bone amounted to five (5) inches four (4) lines Paris measure.\* The distance of the right from the left side, in a horizontal plane drawn in the upper angle of the occipital bone, four (4) inches seven (7) lines. The distance of the vertex from the horizontal plane, on which the rounded end of the remaining mastoid process of the right side of the skull rested, (the left mastoid process was wanting), was five (5) inches four (4) lines; and the distance of the vertex from a horizontal plane, drawn close under the left zygomatic process of the frontal bone (the right zygomatic process was wanting), was four (4) inches and three (3) lines. The frontal bone and the arch of the occipital bone ascended very directly in height, were arched in a moderate degree, and were greatly elevated in proportion to their breadth. This was also the case with the parietal bones, which presented a greatly-elongated oblong dimension.

The vertical height of the left parietal bone amounted to three (3) inches eight (8) lines; and its breadth near the middle, where it was smallest, to two (2) inches and nine (9) lines. The squamous portion of the right temporal bone advanced forwards with an obtuse angle to a good distance in the superior direction. The left temporal bone was wanting. The left side of the cranial cavity was much shorter than the right, and in general both lateral halves were remarkably symmetrical.† But the direction of the frontal plate of the left orbit was most remarkable in this respect, namely, that the orbital margin, and in general the whole orbital portion of the frontal bone, was directed in an unusually great degree, from within and above, outwards and downwards. On the right side, that part of the frontal bone which contributes to form

\* For explanation of these measures, see this Journal, volume lxx, p. 497.

† According to Blumenbach's account, the *Makrocephalus* which is represented in the *Decades* was of a very symmetrical shape. But if we judge from the figure, it appears to be on the right side considerably shorter than on the left side.

the orbital cavity was wanting. I must, however, observe, that the shape of the fragments hence indicated, that the right side of the face might have advanced further in the anterior direction than the left side. All the sutures, with the exception of the frontal, were very distinct and well formed.

Of a second Makrocephalic skull, a male, I saw only the frontal bone. This was everywhere very thick, still smaller than the frontal bone of the one first examined, arched in the longitudinal direction, but extended almost entirely straight; in other respects, very much elevated in proportion to its breadth. The superior margins of the orbits were, as the skull of that *Makrocephalus* which Blumenbach has delineated, almost entirely horizontal.

As, according to the statements which were made to me by some persons in Kertsch, in that town so unusually a shaped skull as that I have now mentioned has been repeatedly found, and on one occasion several were found together, I cannot justly entertain any doubt, that they do not indicate any thing morbid and taking place abnormally in a few individuals, but that they depended on the peculiarity of a nation or tribe, who at one time inhabited the region in which these burrows are found. But if this has been the case, this nation must have been in a very low stage of civilization, since among the remains of these bones there has been found not the slightest trace of ornaments, domestic utensils, and similar articles, not even the remains of a coffin, although in the lands in which these remains of bones are deposited, coffins and multiplied other articles, which the Greeks have buried with their bodies, are in a state of very good preservation. Most probably, this nation belonged to the primitive inhabitants of the Krimea, at least of the east part of that peninsula, and separated from Asia by a small strait of the sea. The foundation for this idea is furnished by the celebrated work of the ancient master of medicine, Hippocrates, which treats of the influence of air, water, and soils. He there states, in the section in which he treats of Asia, that, in the district which extends directly from the countries where in summer the sun rises to the *Palus Maeotis*, among others dwells a race, the individuals of which were named *Makrocephali*, or long-headed. And this name they bear, because there is found no other tribe who possess similarly shaped heads.

Originally, the cause of the elongation of the head was, as it appears, a custom among the people; but, eventually, nature had also contributed to this shape. This custom, which arose from the idea that the more elevated the head, the more noble is the individual, consists in this, that, after the birth of a child, its head, while it was still tender and compressible, was compressed and moulded by the hands, and, partly in this manner, partly by



means of bandages and appropriate machines which resisted the natural spherical shape of the head, was compelled to grow particularly in the long and elevated direction. In this manner, therefore, a peculiar custom impressed on the head a peculiar direction of growth. In the course of time, nature accommodated herself to it to such a degree, that afterwards the practice became entirely unnecessary, as the skull took that shape of itself.\*

Pliny the Elder, who, in his natural history, in like manner mentions the *Makrokephali*, places them in a tract of territory westward from Trapezus, the modern Trebizond, in Asia Minor.† In this same region, also, are they placed by Pomponius Mela, who denominates them people of uncivilized manners (*mores in conditi*).‡

It is, however, remarkable that Xenophon, who, in his celebrated retreat, came through these countries, and gives expressly, in his *ANABASIS*, the transactions which he had with the *Makrones*, through whose territory he marched, and whom several learned men have considered to be the *Makrokephali* of Hippocrates and Pliny, has made not the smallest mention of any race who were distinguished for the unusual figure of the head.§

In a manner different from that of the *MAKROKEPHALI*, the *Derbikes* and *Sigygnæ* tribes of the *Kaucasus* must have changed the shape of the heads of their children. Strabo mentions of them from hearsay, that they laboured to give the head the greatest possible elongation in figure, desiring a forehead so prominent, so that it projected beyond the chin.||

When we compare the above-mentioned passages from the works of ancient authors with the facts which I have ascertained upon the rare presence of well-formed skulls in Kertsch, it results that the nation or race, whose individuals were named by the ancients *MAKROKEPHALI*, have been still more extensively distributed than ancient authors admitted; and, in like manner, that, at one period of antiquity, which must have been very remote, and, in certain countries of Asia and Europe, one of the most singular, and, in some respects, ridiculous customs, to which vanity and pride have impelled the human race, have prevailed and operated on the human figure in such a degree, as has been observed in modern times only in America. That this custom was prevalent in America at the time when it was discovered by the Europeans, and that several nations of that continent were in

\* Hippocratis Opera, editio Kuhn, Lib. i. p. 550.

† Plinii Historiæ Naturalis, Lib. vi. Cap. iv.

‡ Pomponius Mela, Lib. i. Cap. xix.

§ The history of the meeting with the *Makrones*, (*Μακρονες*), and the treaties with them, is given in the fourth book of the *Anabasis*. *Κυρου Αναβασις* A. Edit. Hutcheson, 1813, p. 277.

|| Strabonis Geographia, Lib. xi. Cap. xvi.

the habit of giving the heads of their children, by the employment of means which acted by compression, a form deviating from natural, particularly the cerebral part of the skull, is an ascertained and undoubted fact. The consequences of this practice are shown by the collection of skulls, and the *Decades Craniorum* of Blumenbach. Here it may be observed that the skull of the Illinois, and some of the early inhabitants of Peru, present shapes very similar, and shapes artificially produced, as those of the ΜΑΚΡΟΚΕΦΑΛΙ of Hippocrates and the environs of Kertsch, whereas those of the Caraihs are unusually strongly compressed from above downwards, and are elongated from before backwards in a great degree. Similarly shaped skulls to those of the Caraihs have been recently found in very considerable quantity in Upper Peru; and among these, only those which had belonged to very young children showed a remarkable degree of flattening.

[The author here alludes to several of the skulls which are described by Dr Morton, and of which accounts are given in the fifty-fourth volume of this Journal;\* the accounts of M. Alcide d'Orbigny, in his work on the American race;† and those given by M. Tschudi, in a paper in Müller's Archiv. for 1844. In this paper, M. Tschudi gives a short sketch of the leading forms of the ancient Peruvian skull, and promises to give full and detailed descriptions in a subsequent work.]

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**ART IX.**—*On Anæsthesia and Anæsthetic Substances generally; being an Experimental Inquiry into their Nature, Properties, and Action, their comparative Value and Danger, and the best means of counteracting the effect of an overdose.* By THOMAS NUNNELEY, Esq., F. R. C. S. E., Senior Surgeon to the Leeds General Eye and Ear Infirmary, Lecturer on Anatomy, Physiology, and Pathology, in the Leeds School of Medicine, &c. (Transactions of Provincial Medical and Surgical Association, vol. xvi.)

**CHLOROFORM.**—Not only have I placed chloroform first in the list of agents whose effects are related, but also a greater number of experiments have been made with it than with most of the other substances. Inasmuch as it is that body which is now used almost exclusively in practice, it has attached to it more interest and importance than many of the others. It is, of all the anæsthetics which I am acquainted (with one exception), one of the most, if not the most, agreeable, and at the same time it is pos-

\* Edin. Med. and Surg. Journal, liv. p. 443. Edin., 1840.

† L'Homme Americain De l'Amerique Meridionale. Par Alcide d'Orbigny, Chev. de la Legion d'Honneur. Deux vol. 1838-1839. 8vo.

sessed of very considerable power. It is for the most part pretty constant in its action ; but I am also bound to add, it is one of those which require more care in its administration than most of the others, and I would venture to say much more care than has always in practice been exhibited. It is true, that many fatal cases in which it has been given are known, but, I am firmly convinced, there are many more fatal cases which have not been reported. Even in those cases which have come to light, all sorts of explanations, and attempts to explain away the true cause of death, have often been attempted, rather than admit the fact of the person having been killed by the chloroform. This is, perhaps, scarcely to be wondered at ; the admission of the fact would be almost like acknowledging being accessory to the death of the patient, which most men naturally shrink from. Careful examination, however, of the preceding experiments, cannot but convince any impartial person that, in reality, death in these cases was caused by the chloroform ; either from some abnormal condition or peculiar idiosyncrasy, by which the person was rendered unusually susceptible to its influence ; or, as I believe, far more frequently from a want of that experience and care in its administration, which the exhibitor should have possessed and attended to. When I see statements by medical men, that they have “ boldly administered ” chloroform in quantities of from six drachms to an ounce and a half at a time without any ill effects, I cannot but congratulate both the patient and the doctor, that the rashness in employing such quantities has been equalled by the carelessness in administering it, so that by far the greater portion of the fluid has been wasted by escaping into the air of the room, rather than passed into the lungs of the inhaler, and thus the carelessness of the administrator has, contrary to what generally occurs, and which he would do well not to risk again, been the means of escaping from the effects of his rashness.

Two very important practical points, in reference to the administration of chloroform, appear to be proved by these experiments.

1st, That the effects produced upon similar animals by like quantities of chloroform, all circumstances, so far as can be ascertained, being precisely similar, are not identical, the effects being far more considerable in one than another : even upon the same animal the effects are far more decided at one time than they are at another. This, a comparison of experiments 1, 7, 9, 10, 11, and 16, with experiments 5, 6, 12, and 15, will demonstrate ; and, especially so far as the different effects upon the same animal are concerned, is shown most unequivocally in experiments 11 and 12, and in 16 and 17.

2d, That the intensity of effect appears much rather to depend upon the quantity of chloroform inhaled in a given time,—the

concentration of the vapour, than upon the absolute quantity inspired. Thus a small quantity suddenly and quickly inhaled for a short space of time will produce, not only more rapid and decided effects, but also far more dangerous, than a far larger quantity will do, if taken in more slowly, and consequently in a less concentrated form. Many of the experiments show, that when the creature inspired fully and deeply, and the temperature was high, so as to ensure a speedy conversion of the chloroform into vapour, anæsthesia was not only quickly produced, but was also most decided; also that the effect was more prolonged if the animal recovered, and in many cases terminated in death. In the 13th experiment the vapour was not concentrated, but the inhalation was more prolonged; in Nos. 14 and 15 the vapour was more concentrated, but the inhalation not so prolonged; in No. 13 recovery soon occurred; in Nos. 14 and 15 death was occasioned. The same fact is illustrated with the three carp, Nos. 25 and 26, and with other creatures.

**ALCOHOL AND SPIRITS OF WINE.**—At first my experiments were made with spirits of wine, 64 over proof, or containing about 90 per cent. of pure alcohol; I subsequently procured alcohol as nearly as possible pure, the specific gravity of absolutely pure alcohol being, at 60°, 0.796, while that which I used was 0.797, so that for all practical purposes it may be considered pure. Now, although it differs from ether only in containing the elements of an atom of water, being considered by chemists a hydrate of the oxide of ethyle, and agrees with this fluid in many of its chemical and physical properties, yet it differs materially in its physiological. It has no pretension to rank as an anæsthetic. When taken by inhalation, its vapour produces very little effect, while the fluid itself, put into the stomach, produces considerable, being the reverse of the action of true anæsthetics; as, for instance, chloroform and the chloride of olefiant gas, which within the stomach produce less effect than even spirits of wine, but by inhalation, infinitely more.

**SULPHURIC ETHER.**—If ether be not so powerful in its effects as chloroform, it is certainly not so dangerous in its application; a larger quantity is required to produce a like condition of insensibility, and more time is occupied; the system is not so speedily brought under its influence. In the commencement of the inhalation, it is necessary to be very careful not to allow the air to contain too large a proportion of the vapour, otherwise so much irritation of the glottis and bronchial membrane is occasioned, and the respiratory muscles so violently excited to action, that the continuation of the inhalation must be, at least temporarily, suspended, sometimes even permanently. Another circumstance is, that before the animal is fully brought under its influence, there

is some excitement, which, at times, is very considerable; so much so, as to render a patient wholly unmanageable, the only method of calming him being to place the system more fully, and as rapidly as may be, under the influence of the ether; when, however, this is accomplished, if not carried too far, so much depression is not produced as by chloroform, and I think the condition of insensibility may be longer continued without danger when ether is used than when chloroform is. The action of the heart is not rendered so excessively feeble and rapid, and disappears sooner than after chloroform.

To counterbalance this, feverishness, heat, and pain in the head, are more apt to follow. While the action of ether upon the system is, I believe, of essentially the same nature as chloroform, it, in the first instance, is certainly more of an excitant than chloroform, and, as stated, if not used in very large and unnecessary quantities, or very long continued, it does not occasion such depression of the system; hence, in cases where there is reason to suspect any peculiar susceptibility to the action of the depressing influence of anæsthetics,—such as disease of the heart, of an asthenic character, or considerable constitutional debility,—if it be determined to use one of these agents, ether should, I think, be selected in preference to chloroform. So also when the operation itself is likely to produce a considerable shock and depression of the system, or where it is likely to be very long and tedious, then it may fairly be well considered whether ether may not be the safer of the two agents. On the other hand, when the patient is plethoric, or very excitable, or in whom there is a tendency to fever,—where there is any affection of the head, whether of the brain or its membranes, or predisposition to congestion,—any asthenic disorder of the heart, or disease of any kind in the lungs; if it be, notwithstanding these contraindications, from any other consideration, determined to use any anæsthetic agent,—then ether is not the one to be selected on account of its stimulating property.

**SPIRIT OF NITRIC ETHER AND NITRIC ETHER.**—Previous to trying the effect of nitric ether itself, it appeared well to ascertain if the spirit of nitric ether, which appears to be an indefinite, or rather undefined, compound of hyponitrous or nitric ether, alcohol, and water, in proportions not ascertained, possessed any anæsthetic property. The two experiments, Nos. 57 and 58, were such in their results as to show that it cannot, in any moderate quantity, or when inhaled for a moderate period, be considered as possessing much, if any, of this property.

Nitric, nitrous, or hyponitrous ether, is a fragrant and by no means disagreeable substance, when first inhaled. If the vapour be not diluted, it produces some degree of irritation and distress;

but this soon disappears, when the inhalation is continued without any objection or indication of distress or dislike. It certainly possesses considerable anæsthetic powers, which, however, are not equal to those of chloroform, or even sulphuric ether, and the anæsthesia is more evanescent in its character. A state of total insensibility may be easily induced, and maintained for some time with safety; but the prolonged inhalation of it, even when the vapour is not concentrated, is dangerous, and may be fatal to life, as in experiment 276. In addition to the objection founded on the evanescence of the anæsthesia produced by nitric ether, there is another which, in practice, would be found of a formidable, if not insuperable, nature,—namely, the instable character of this ether. It very soon, especially when any air is in contact with it, undergoes decomposition; it becomes quite acid and more irritating, nitric oxide gas being given off, alcohol, nitrous, and acetic acids remaining. It therefore is not likely to be of much practical utility as an anæsthetic.

**ACETIC ETHER.**—Acetic ether possesses a warm, aromatic, agreeable odour. When the vapour is diluted, it is pleasant to inhale; but when concentrated, it produces at first considerable irritation, as shown by the flow of saliva and tears, and the violent attempts to escape from its influence. This, however, subsides, and the respiration becomes more free. When given as a fluid internally, it produces great distress. I put not more than ten minims upon my tongue; instantly it appeared to penetrate into the substance of it, and I was seized with great distress and a sense of suffocation, as though the respiratory muscles would cease to act, arising, I think, from the vapour in the mouth and pharynx. I am certain none was swallowed, but the stomach immediately became distended, and a very large quantity of flatus was eructated. After walking about for two minutes, a slight perspiration broke out, and the eructation subsided, but there continued a sense of dryness and constriction about the fauces; the pulse was languid, and there was a feeling of weakness, which passed off after a few more minutes; the head was not in the least affected. Though acetic ether certainly possesses considerable power as an anæsthetic (experiments 68 and 69), and is far more stable in its character than nitric, it is not likely to be used in practice, as it is inferior to several other substances.

**CHLORIC ETHER, CHLOROFORM, AND SPIRIT OF WINE MIXED.**—From the resemblance, in many of its qualities, of chloric ether to chloroform, I thought possibly it might possess the good properties of this without its depressing effects; and if so, that it would prove an useful agent. The effect of the experiments, Nos. 79 to 85, will show that, to a great extent, this idea is not ill-founded. Chloric ether produces less irritation

than the other ethers; indeed, there are none of those violent attempts to escape as with nitric and acetic, or even sulphuric. It is very pleasant to the smell and taste, and is freely respired. In none of the animals were any convulsions produced, nor any unpleasant effect, while the degree of insensibility was sufficient for any purpose. This, so long as the vapour was inhaled, continued, but afterwards speedily passed off. It has for some time been used internally as a valuable medicine, and, did we not possess so safe, and, at the same time, so powerful an anæsthetic as we do in the chloride of olefiant gas, would, I consider, be well worth a fair trial in practice; even as it is, were neither it nor chloroform at hand, I should not for a moment hesitate in using chloric ether instead. The experiment upon the cat, No. 85, will show how large a quantity of the ether, and how much time was required, in order to destroy the creature; and the *post mortem* appearances will show the condition induced in the various important organs. Experiment No. 84 was instituted for the purpose of ascertaining if a mixture of chloroform and spirit of wine would produce the same effect as chloric ether; the results show, that though chemically chloric ether may be regarded as a mixture of the two bodies, yet that the relation which they bear to each other in chloric ether is not the same as when they are mechanically mixed, and that the effects are not so satisfactory.

**HYDROCHLORIC ETHER.**—Hydrochloric ether, or the chloride of ethyle, is a pleasant ether of a very feeble odour, and is soon converted into vapour. Its power as an anæsthetic is very low. It is more volatile than most of the ethers, but is far less aromatic and pungent; as in odour so in effect, it more resembles alcohol than any other ether.

**HYDRIODIC ETHER.**—The iodide of ethyle is a colourless, ethereal fluid, of a strong disagreeable odour, of the same character, but not so intense as the bisulphuret of carbon: it also calls to mind phosphorus, especially after a short exposure to the air. In taste, it is not so pungent as many of the ethers, but strongly phosphorus-like. It does not excite much irritation when inhaled, though it appears to be disagreeable. The experiments show, that whatever anæsthetic property it might possess (and this is not insignificant), it never could be employed in practice, as its action is so very deleterious. Out of the four animals experimented upon, the three which were rendered insensible all died, and the fourth had not sufficient of the vapour to render it in the least insensible, and yet for two or three days it was doubtful whether it would recover. The poisonous property, the smell, taste, and strongly luminous character, made me conjecture that it was not a pure article which I used; Messrs Gale, Baker, and Co., of London, who sent it, assured me it was made by a

"first-rate chemical house," and was perfectly pure; but as I still suspected it, I requested the name of the makers, which I give, Messrs Heathfield and Burgess, Finsbury, London. Like the bromide of olefiant gas, its immediate effects are not so dangerous as the consequences of inhalation in a few hours are; even where sufficient to produce insensibility has not been used, and the animal to all appearances has been perfectly well, death will supervene. As this does not arise from irritation or inflammation of any organ, nor from paralysis of any part, it may probably arise from the blood being poisoned. In one instance, the odour of the ether was distinctly perceptible in the brain, twelve hours after the inhalation had taken place.\*

**HYDROBROMIC ETHER.**—The bromide of ethyle is a pleasant, rather fragrant ether, not of very penetrating smell, is sweetish to the taste, at first rather insipid than not, but afterwards it is more pungent. It possesses very considerable anæsthetic power. Its inhalation does not produce irritation, and does not appear to be unpleasant. When not used in large quantities, the animals soon recovered from a condition of complete insensibility without any disagreeable symptom; and when given, as in No. 98, in a full dose, the creature sank down without moving a muscle, merely from its weight, into a state of the most profound anæsthesia, within one minute after being put into the jar. The fact of respiration continuing at all during fourteen minutes in such a condition, shows that this fluid is more manageable than some others.

**DUTCH LIQUID—CHLORIDE OF OLEFIANT GAS.**—This is not only an aromatic and very pleasant ethereal fluid, but a most valuable one; indeed I believe one of the most valuable anæsthetics yet tried. In general properties, it much resembles chloroform, but in composition it differs materially from it, in containing a much less proportion of chlorine and more hydrogen, as compared with carbon, in an atom; and its boiling point is very different, as a reference to the heads of the respective experiments will show.

A comparison of the experiments with chloroform and with the chloride of olefiant gas will show, that in no one respect is the former fluid superior to the latter. The animals were rendered perfectly anæsthetic in quite as short a time, showed no uncasiness while passing into this condition, remained perfectly still while in it, and, on recovering, were altogether free from any unpleasant symptoms. The condition of anæsthesia may be rendered as profound as possible, and quite as prolonged as can ever be requisite in any operation; and, so far as I can judge, there

\* See also the four experiments made with perfectly pure hydriodic ether, obtained from Mr Bullock, page 315.



is not so much weakness and depression induced as by chloroform ; indeed I think it probable that animals rendered so utterly senseless by chloroform as Nos. 102 and 104 were, would not have recovered. While the large quantity required in No. 108 to destroy the cat, when I wished to kill it, shows that if as small a quantity of it as of chloroform will induce an anæsthetic state, a much larger quantity is required to destroy life, and, consequently, that the chloride of olefiant gas is by so much the safer of the two fluids.

The seven cases mentioned, in which it was given to patients during operations with perfect success, are sufficient to show its safety ; but I have since given it, in several other serious and important operations, to two women in labour, and several times it has been inhaled for experiment, so that no doubt can remain as to its value. When I used it, I was not at all aware it had previously been tried ; but both Drs Simpson and Snow had tried it, and were led to consider it a dangerous preparation which could not be used. This error, I believe, arose from their using an impure preparation, one which possibly contained free chlorine, which would account for the irritation induced, and must always be avoided with the utmost care,—a point, however, upon which there can be no great difficulty, as I have used chloride of olefiant gas perfectly pure, prepared by three different chemists.

**BROMIDE OF OLEFIANT GAS.**—Having ascertained that the chloride of olefiant gas acts so pleasantly as an anæsthetic, I was desirous to ascertain if the analogous compounds of iodine and bromine possessed similar properties. The iodide being a crystalline substance (corresponding with iodoform), is not adapted for the purpose ; I therefore only obtained the bromide.

It is, of all the substances I have tried, one of the most agreeable and pungent ; its odour strongly resembles that of the flower of the heliotrope. In taste, it is one of the sweetest bodies I know, more so than sugar ; for if the tongue be touched with it, and sugar be afterwards put into the mouth, the sweetness of the bromide can be perceived above that of the sugar. It does not produce any irritation of the tongue. The specific gravity of the fluid is considerable, and the density of the vapour is great. It is a fluid of a very pale straw colour. It is inhaled without irritation or any repugnance. Though it produced insensibility before the animals were reduced to that state, the respiration was most laborious ; and though all appearance of distress speedily disappeared on the animals being released, in a few hours afterwards they all died. In this respect it differs from other substances ; immediately after the experiments, the animals appearing well, and soon after dying,—effects which would more justify the opinion of the blood being poisoned by it than (perhaps, excepting

hydriodic ether) with most of the other substances tried. With them, if the immediate effects pass away, the animal may be considered safe; not so with this, as the experiments will show.

**OLEUM ÆTHEREUM.**—When I first tried this substance, which was amongst those first used, I thought I had discovered an agent likely to be very useful, as it appeared to possess more power than any of the ethers, and yet to be safe. It happened that the experiments first made were those in which the greatest insensibility was induced, hence (as other experiments did not confirm the first) the number made, in order to ascertain the true value. More extended observations have shown that it possesses but doubtful, if any, superiority over sulphuric ether; indeed, it is not improbable that it owes its property to the ether which it contains. This appears to be in uncertain proportions, and the effects are not uniform. In some animals the insensibility was profound, as in No. 118, while in others it was very little. Out of three instances where I administered this fluid to patients, in one, considerable excitement was produced, and the man could not be prevailed upon to continue the inhalation, not from any dislike to it, or from any other unpleasant effect, but merely from his excited, half-unconscious, self-willed condition; in the other two persons, very little effect was produced. It is pleasant and very aromatic; this probably arises from some volatile oil combined with it. This aromatic property led me to the supposition, that it might produce less depression than chloroform. The less specific gravity of these three specimens which I used than that stated in the *Pharmacopœia* and by chemists, as well as the less anæsthetic effects produced by the latter of the specimens which I obtained, shows it to be a compound of very uncertain proportions, and therefore not to be depended upon, even were its powers greater than they are.

**HEAVY OIL OF WINE.**—Although this substance is closely allied to the ether group, from the decomposition of which it is formed, and it is a rich hydrocarbon, as an anæsthetic it is of no value, probably, for two reasons;—first, its combination with an acid; and second, from its not being very volatile. It is true, that in *Turner's Chemistry* it is described as being without action on dry vegetable colours, and as a colourless fluid, in neither of which particulars did the specimen I tried agree. That which Mr Morson prepared for me possessed a very acrid, irritating character. The explanation of this discrepancy I have already stated. It may be said, the quantity I tried was not sufficient to produce a decided effect. This may be true; but had a larger quantity been used, the irritation would have been too great to have been borne. If, therefore, its volatility were sufficiently great to give it rank as an anæsthetic, its unstable and uncertain

composition, and irritating quality, at least when so changed, would prevent its useful employment.

**ALDEHYDE.**—According to modern chemists, aldehyde is the analogue of alcohol, being the hydrate of the oxide of acetylene, as alcohol is the hydrate of the oxide of ethylene. It unquestionably possesses considerable anæsthetic powers, but it is not likely to be employed in practice. Its vapour is of a singularly irritating and suffocating character, producing, even when inhaled for only a short time and in a diluted condition, very unpleasant sensations; and evidently, when inhaled in a more concentrated form, as shown by the violence of animals in attempting to escape from it, a most distressing effect.

When used in comparatively small quantities, it occasions great distress, without loss of sensation or consciousness; and when used in sufficient quantity (though the quantity required is not very large) to induce unconsciousness and insensibility, it is dangerous, death being likely to supervene, as in No. 144, though possibly the pregnant condition of this animal might conduce to this effect, but which is not very evident.

It is a very curious circumstance, when so much irritation is produced by the inhalation of aldehyde, and also of several of the other fluids, to observe how very quickly this subsides, without any ill effect; and that when the animal has died while breathing them, on an examination the bronchial membrane is usually found uncongested, rather pale than not, and without any increased secretion.

The facts of the continued pulsation of the fœtal umbilical arteries after the maternal heart and circulation had entirely ceased, and the body of each fœtus had ceased to move, though the uterine fibres and maternal intestinal muscles continued to contract, are physiological facts worth bearing in mind.

**MIXED SUBSTANCES.**—As no particular advantage appeared to result from the combination of substances, whose effects are related under this head and amongst other experiments, as Nos. 49, 71, 77, 153, 4 and 5, 179 to 184, 214, and others, it will be unnecessary to detain ourselves with any observations upon them.

**IODOFORM.**—Iodoform is a substance but very little known to the profession. It is thus described in the eighth edition of *Turner's Chemistry*, page 1010 :—"It crystallizes in pale yellow shining scales, having a slight but disagreeable odour, resembling that of saffron, and adhering strongly to the hands or to substances brought in contact with it. It is insoluble in water, very soluble in alcohol, ether, and pyroxylic spirit. It sublimes at 212°, and at 248° it is decomposed, yielding iodine, carbon, and hydriodic acid."

As in constitution it is exactly analogous with chloroform, the three atoms of chlorine being replaced by three atoms of iodine, I thought it worth trying, and accordingly requested Mr Morson to prepare me some, which he was good enough to do. The general appearances of it correspond pretty nearly with the description in Turner, except that the scales have only a faint yellowish tinge, perhaps from its purity; they have a soft pearl, somewhat glistening, appearance; the odour is most peculiar, very penetrating, and extremely permanent, far more than that of most substances. From its penetrating odour, I at first used it with great caution. This, however, was needless, as it appeared to produce hardly any effect, whether by inhalation or by the mouth; indeed, when placed upon my own tongue, very little sensation was produced. This I attribute to its insolubility and unvapourizability. It will remain exposed to the air for days, without any apparent change, and may likewise be exposed to a temperature of  $150^{\circ}$  for a considerable period without any effect being produced, except its becoming of rather a deeper yellow; yet it cannot be absolutely fixed, inasmuch as its vapour is very perceptible, and adheres to everything. By Dr Turner it is stated to be very soluble in alcohol, ether, and pyroxylic spirit. I thought to procure its effect by giving it dissolved in one or other of these fluids, and thus get it vapourized; but on trial, I found it to be nearly insoluble in these. Of this I informed Mr Morson, whose statement agrees with that in Turner; but still, in my hands, the fact is as stated, though Mr Morson assures me the specimen is pure, and was precipitated from alcohol. Possibly it may be formed by the mixing of two substances dissolved in this fluid, and yet, when formed, not itself be very soluble.

Either, then, iodoform does not support the idea which I have before thrown out respecting the doctrine of substitutions,—or the crystalline and fixed character of the substance, differing so widely as it does from its analogue in composition, chloroform, shows that with substitution of elements we must also have, as indeed would be expected, isomorphism, otherwise the analogy in action upon animal bodies will not be preserved any more than analogy in physical and chemical characters.

**OLEFIANT GAS.**—Heavy inflammable air, bicarburet of hydrogen, or olefiant gas,—by one or other of which names it for long has been known,—is a pure hydrocarbon, containing the two substances in equal ratio, and is therefore, from its gaseous form, as well as its composition, a very favourable substance for determining if these two substances, in combination alone, are capable of producing anæsthesia. The result of the experiments is in the affirmative; that they are capable of inducing this state. The *post mortem* appearances also confirm the similarity in action.

There is one point connected with these appearances which is very curious, and which may also be observed with some of the other hydrocarbons, viz. that when the colour of the blood is found to be altered, the effect is seen not to be in rendering the blood darker, but more florid. To this we shall again advert ; but when the *rationale* of respiration is taken into consideration, it is an effect which few would, *a priori*, be prepared to find.

Though olefiant gas is an anæsthetic, and consequently is of considerable importance in elucidating the action of these agents, it is not one of those which are likely to be used in practice. Not only would there be the difficulty and trouble in preparing and preserving it, but it is not a safe and manageable agent ; since, when employed in innocuous doses, the anæsthesia is not complete ; when used in larger, death may not improbably be caused. Ten per cent. of the gas does not appear to produce such a degree of insensibility of any permanence as could be depended upon in a painful operation of even a moderate length ; 15 per cent., though causing total insensibility, also induces a dangerous condition ; while life is speedily destroyed by from 20 to 25 per cent.

**COMMON COAL GAS.**—Common coal gas, when prepared with adequate carefulness, consists principally of the light and heavy carburetted hydrogen gases ; “ but, besides these ingredients, an inflammable vapour, free hydrogen, carbonic acid, carbonic oxide, and nitrogen gas. The discoveries of Mr Faraday have elucidated the subject still further, by proving that there exists in oil gas, and by inference in coal gas also, the vapour of several definite compounds of carbon and hydrogen, the presence of which, for the purpose of illumination, is exceedingly important.”

Coal gas is not only a powerful anæsthetic, but, so far as my experiments go, it is also a very safe and manageable one. The animal is quickly brought into a state of unconsciousness and insensibility, and that without the least appearance of suffering. It does not act as an irritant ; it does not excite cough ; there is no repugnance to the inhalation of it ; and the condition of anæsthesia may be safely maintained for a period sufficiently long to allow the performance of most operations.

It appears to deserve a full and fair trial, and if in practice it should be found, as I believe it will, to be successful, the cheapness of it would be a recommendation not unworthy of attention, should its gaseous form not prove too inconvenient. It affords another instance of a hydrocarbon producing an unnaturally bright condition of the blood.

**BENZOLE.**—Benzole is not nearly so pleasant in smell or taste as chloroform or Dutch liquid, the general appearance of which it resembles. It has a strong smell of coal naphtha, or common

coal gas. It is a powerful anæsthetic, and though during its inhalation the animal appears much distressed, it is one of the safest. I have seen animals recover from apparent death after its use, when I had not the least expectation of their so doing. The anæsthesia produced by its inhalation is profound, but before becoming insensible, there generally are the most remarkable muscular twitchings over the whole body. At first these merely amount to trembling, though they sometimes resemble an attack of ague; but as the unconsciousness increases, these movements assume more the appearance of convulsions. In experiment 323, where the frog and newt were exposed to the vapour of benzole, they appeared dead, but subsequently became quite well. Here the first indication of life, especially in the frog, was a sudden start of the limbs, occasioned by taking hold of one leg. I should not have known the movements from those excited in a dead animal by galvanism.

Though a powerful and safe agent, this distress, and the oppression in breathing occasioned by benzole, will prevent its employment in practice. It affords an illustration of the action of an agent, where carbon is in the proportion of two atoms to one of hydrogen.

**CAMPHOR.**—Camphor is one of the numerous forms to which the combinations of carbon and hydrogen give rise, as stated in the experiments. According to its ultimate analysis, its composition, so far as proportion of elements is concerned, does not differ very materially from that of sulphuric ether, a more essential difference being apparently in the mode in which the atoms are united to constitute these respective bodies. In ether, 5 of hydrogen, 4 of carbon, and 1 of oxygen, are united to form 1 atom of ether, whereas it is supposed 10 atoms of carbon, 8 of hydrogen, and 1 of oxygen, are united to constitute 1 of camphor; hence they are not strictly isomeric, nor do they produce the same effect upon animals. It is one of those substances which appears to have greater effect when taken into the alimentary canal than when inhaled. Its solid form, perhaps, conduces to this. It is difficult, without employing more heat than could be borne, even when the camphor is dissolved, to get a sufficiently concentrated vapour, in a pure form, to try its effects fairly; and, consequently, we rather get its stimulant property, which appears to be the primary action of nearly all of these triple compounds, as shown, both when the dose is not sufficiently strong to cause complete anæsthesia, or before this state supervenes when the dose is larger. The symptoms caused by a poisonous dose of camphor, taken into the stomach, would confirm this view.

**НАФТНА.**—Naphtha is stated to be a pure hydrocarbon; indeed, that which is called potassium naphtha must be so, since

it is used for the preservation of this metal. The effects of three kinds were tried, wood or pyroxylic spirit, potassium or mineral, and coal naphtha. Of the three, the latter is the only one which possesses much anæsthetic power, and this not to such a degree as to be practically useful. Though it will induce loss of consciousness and sensation, it also produces so much distress, convulsion, and weakness, as to make its employment painful. Upon fishes, its effect is very similar to that of hydrocyanic acid. Though the specimen I tried was rectified, and stated to be perfectly pure, its odour strongly resembled that of coal gas. Possibly, its more powerful action than the other varieties, depends upon some substance which it still retains in solution. Potassium naphtha is rather more powerful than wood naphtha; but both produce, especially potassium naphtha, great distress, difficulty of breathing, and weakness, yet have very little anæsthetic power, —too little to be of any use.

**OIL OF TURPENTINE.**—Oil of turpentine is another of the pure hydrocarbons, and appears to be composed of the same number of atoms of hydrogen and carbon as camphor, being minus the one atom of oxygen. The experiments show it to be a decided anæsthetic, but one which is by no means likely to be used. It occasions very great distress; the convulsions are excessive, and it apparently affects the brain in a great degree, producing, so far as can be judged, excessive giddiness; indeed, this effect is perceptible to most persons in inhaling it, even in a very dilute form; nor can unconsciousness be produced without danger to life, as No. 198 shows. How far the convulsions may be dependent upon the cause before alluded to under the head “camphor,” of want of sufficient concentration in the vapour, the boiling point of turpentine being as high as  $312^{\circ}$ , is doubtful.

Many of the essential oils are stated to be identical in composition with the oil of turpentine; but it does not appear necessary to try their effects.

**CREOSOTE.**—Creosote is another of those substances which, taken into the stomach, act very energetically; when inhaled, it produces, comparatively, little effect, probably on account of its fixed character. It cannot be regarded as an anæsthetic; even were it so, its effect upon the lungs, in occasioning pulmonary apoplexy, would prevent its employment. It is curious that so little congestion of the bronchial membrane should result, even from its lengthened inhalation, while such great irritation is produced by the application of the liquid to any part.

**PROTOXIDE OF NITROGEN.**—Knowing the effects of the protoxide of nitrogen when breathed for a short time by man, and the statements in physiological and chemical works as to its effects when inhaled, ever since Sir H. Davy’s celebrated experiments

upon it, it appeared to well deserve a trial. Indeed, we might go farther, and refer to the account Priestley gives in his history of the discovery of dephlogisticated air, of putting two mice into it, and also of breathing it himself.\* With its effects I confess myself disappointed.

Had not the experiments been conducted with great care, and the gas made with every attention to its purity, I should have suspected some error; but this I do not believe did occur. (The gas was made by Mr Staniland, who, I may mention, kindly made this gas, the oxygen, and carbonic oxide, for me, and assisted in the experiments with them; while, for the making of the olefiant gas, hydrogen, oxygen, sulphuretted hydrogen, and carbonic acid gas, and assisting at the experiments with them, I am indebted to the kindness of Mr Morley, both gentlemen being among my colleagues at the Leeds School of Medicine.) The gas was pure and well washed, and the experiments were conducted with every care; yet the effects of No. 206 and No. 207 contrast very strongly with those of No. 209 and No. 210, especially with No. 208, wherein death ensued in less than five minutes. They are quite sufficient to show that nitrous oxide never could be employed as an anæsthetic, and that the inhalation of it is not altogether so harmless as is generally stated. Probably, when inhaling it in the ordinary way as a laughing gas, almost as soon as it begins to produce its effect, very little more is taken into the lungs, and insensibility is not very frequently occasioned, but only its first effect, the drunkenness and excitement, though even a very small quantity will quickly stupify some persons. These experiments, however, clearly prove that with animals the state of insensibility, when safe, passes off so quickly as to be practically useless, while if it be rendered more profound or more prolonged, it is highly dangerous. Moreover, they indicate its action to be very uncertain; that of three animals, of the same species, and similar as to age, size, and sex, with the same quantity of gas, one was killed in five minutes, while the others, after being exposed to the gas for nearly three times as long, showed hardly any symptom of being affected by it. The *post mortem* appearances in the cat destroyed by the protoxide of nitrogen may also be referred to, as showing that the action of it differs from that of the hydrocarbons and their compounds, the condition of the heart and lungs being very different. In animals destroyed by them, the lungs are generally much collapsed, showing, I believe, but little action upon their structure. The less the lungs depart from a natural condition, the more they will, in my opinion, be shrunk on the thoracic parietes being opened, the pressure of the air being then equalized.

\* Experiments and Observations on different kinds of Air, vol. ii., pp. 44 and 100.



If there be no unnatural deposit or effusion, or the cells be not emphysematous, then the natural resilience of the lungs comes into play ; whereas, when from any cause the structure is altered, this cannot take effect, and consequently the lungs are more bulky ; but they do not present this appearance after death from nitrous oxide. The condition of the heart, also, especially of the left ventricle, materially differs. It is curious to remark, that with a gas so rich in oxygen, the blood should be dark, while with some of the pure hydrocarbons, where there is no oxygen, it is perfectly florid, a fact which it is very difficult to account for in accordance with the received notions of physiology.

**HYDROCYANIC ACID.**—Very shortly after the introduction of chloroform, in giving it by inhalation to an infant, who was suffering most dreadfully from long-continued convulsions, which had resisted every means I had tried, I was so struck with the resemblance in the symptoms it produced in the child, and the appearance of the animals to whom I had administered hydrocyanic acid, when pursuing the inquiry into the effects of this acid upon animals, that I determined to ascertain, as far as practicable, if the idea then formed was correct. Further inquiry has satisfied me that it is so ; and I have no doubt, from the symptoms during life, and the appearances after death, that the *modus operandi* of these two substances is essentially the same.

Hydrocyanic acid affords a good instance of a hydro-acid, in which the hydrogen is united with a radical base composed of carbon and nitrogen. It is a most powerful anæsthetic,—indeed, the most powerful with which I am acquainted,—its power being so great as to constitute the objection to it, and to render it so unmanageable as, probably, to prevent it being useful in practice. This energy, I suspect, arises from the nitrogen it contains. It would appear that those compounds into which nitrogen enters, as the vegetable alkaloids, exert a more energetic action upon the animal economy than any other substances ; and further, that this action appears to be more specifically upon the nervous matter. Whether this arises from any analogy in composition, or from more readily entering into composition with its substance, or from being endowed, so to speak, with some allied power, force, or energy, by which it is capable of exciting more readily corresponding or opposing phenomena, (whether vibrations, undulations, or whatever other change it may be), as nervous matter itself, would, in the present state of our knowledge of these subjects, be idle to speculate upon ; and, perhaps, the bare mention of an idea, the meaning of which it is difficult to find language exactly to convey, may be thought to be merely idle dreaming and nonsense.

Hydrocyanic acid acts with nearly equal force and rapidity,

however given, whether by inhalation, applied to any portion of the alimentary or genito-urinary mucous membrane, or by injection into the veins, also with great power in contact with the cellular membrane; hence I conclude (considering its composition as well), that it possesses more affinity for, and control over, animal tissues, or some parts of them, than many other substances.

**CONIA.**—Some time back, when wishful to try the effect of a pure hydrocarbon with elements in equal ratio, I asked Mr Morton if he could name one; he said not, but suggested conia as being a volatile alkali. In the last edition of *Turner's Chemistry*, the composition of conia is given as consisting of equal proportions of carbon and hydrogen, it being doubtful if nitrogen is present or not. From the effect of it, I should not entertain any doubt of the presence of nitrogen. The experiment will show it to be of a very destructive character, being most inimical to animal life; that, when inhaled, it acts less energetically than when given by the mouth; and that when unconsciousness comes on, death speedily supervenes. Conia appears to produce intense vertigo; indeed, the symptoms during life, and the *post mortem* appearances, would all indicate that the brain itself is more primarily and powerfully acted upon by it than by any other of the substances tried. In all the animals destroyed by it, not only were the larger sinuses filled with dark blood, but the vessels of the pia-mater, and of the substance of the cerebrum and cerebellum, were much congested. There was also serous effusion.

Conia can hardly be ranked as an anæsthetic. It is undoubtedly a narcotic, and affords a good illustration of the distinction between anæsthetics and narcotics.

**HYDROGEN.**—Though hydrogen gas has been so frequently experimented upon, that its effects upon animals may be supposed to be pretty generally understood as entirely negative, yet, in pursuing this inquiry, it appeared to me to be desirable to make a few experiments with it, for a twofold purpose;—first, of ascertaining the effect of the mere absence of oxygen; and, secondly, as we have seen that a pure hydrocarbon possesses an anæsthetic power, to determine, if possible, if this effect is to be attributed to the compound body, neither the hydrogen nor carbon separately possessing the property; or if either of them does possess it, then to which of the two it does belong.

The experiments are perfectly conclusive, in showing that pure hydrogen does not possess any anæsthetic property whatever, and, as is well known, that it is perfectly innocuous to animals; but I think most persons will agree with me in my surprise at the length of time which an active warm-blooded animal is capable of living, not merely in an atmosphere containing a large proportion of hy-

drogen, as in Nos. 222 and 223, but in one of pure hydrogen. Certainly the statements of physiologists would not lead to the belief that a dog could remain sensible and uninjured (giving off a large quantity of vapour), during the space of an hour, in pure hydrogen.

**CARBONIC ACID.**—As it is impossible to give pure carbon by inhalation, there only remains to give it in combination with oxygen, as carbonic acid, or oxide. These gases it also appeared desirable to give on their own account.

Carbonic acid is certainly not to be accounted an anæsthetic. It produces stupor, and when long continued, death; yet this is evidently the effect of a different cause from that induced by a hydrocarbon; with the acid it doubtless results from the blood being poisoned. The action upon the nervous system is secondary; yet even here, life is maintained longer than is generally supposed. It is stated in the best works on physiology, that when one-tenth part of carbonic acid is present, the air will no longer support life. This statement requires some modification; it should be added, permanently. It is doubtless true, that for any lengthened period it could not support life; but in No. 225 we see an animal remaining altogether unaffected for the space of fifteen minutes, in an atmosphere containing, at first, one-twelfth part of carbonic acid; and, consequently, considerably more before the close of the experiment. In No. 226, a dog lived without loss of consciousness or sensation for fifteen minutes, in an atmosphere containing one-sixth of carbonic acid; and in No. 227, another dog was kept, during fifteen minutes, in an atmosphere containing one-third part of carbonic acid, and yet on removal he quickly recovered. Experiment 227 affords a valuable and satisfactory confirmation of the correctness of the opinion commonly entertained, that where a candle will burn, there an animal can breathe; for in that experiment the candle was *instantly* extinguished, while the dog, although immediately affected, recovered after fifteen minutes' immersion.

**CARBONIC OXIDE.**—Carbonic acid gas, as compared with the oxide, is harmless. This is a violent and speedy poison, even when diluted to such an extent as would render the acid innocuous. The effects of carbonic oxide upon the lungs and the blood are very curious, and may possibly be caused by the oxygen which it contains; but the poisonous effect, when at all concentrated, and its anæsthetic, when sufficiently diluted, must certainly depend upon the carbon; for an animal will live in pure oxygen for some time, and in diluted for any length of time. If carbonic oxide be used in as concentrated a state as 5 per cent., it will produce, in a very short time, dangerous and distressing symptoms. In No. 232, where only  $2\frac{1}{2}$  per cent. was employed,

before insensibility came on, and as sensibility returned, before the recovery was complete, considerable distress was exhibited. This was, to some extent, also observed in No. 234, where not more than 1 per cent. of the oxide was used before the cat became insensible; but when in this condition it remained for twelve minutes perfectly easy and senseless, and also on recovery did not show any signs of distress.

The manner in which the two very young pups supported for so long a period the inhalation of carbonic oxide, affords a curious contrast to the other animals, and is another illustration of the fact, that the function of respiration in the new-born does not all at once assume that importance in the economy which it afterwards possesses.

Though there is no doubt that, when sufficiently diluted, carbonic oxide acts as an anæsthetic, it is not likely to be employed for this purpose. I shall again have to refer to this property; but I would here just call attention to the fact, that of all the agents which we have experimented with, which are possessed of any anæsthetic power, carbon enters into the composition of each one, while every other element may in succession be excluded or exchanged, and yet this property continues.

**BISULPHURET OF CARBON.**—The odour of bisulphuret of carbon is most offensive and filthy, being more like a concentration of that arising from putrefying fish and cabbage than any thing else I can compare it to. The taste is very pungent, but not so disagreeable as the smell. When animals are placed in it, they at first appear to dislike it very much, but this seems in a great degree to subside, and there is no difficulty in their inhaling it. A very copious flow of saliva is induced, but not much cough, and there is no indication of the bronchial membrane being irritated. It might be supposed, from its filthy smell, there would be great repugnance and difficulty in its inhalation, but this is not the case. Though it certainly possesses anæsthetic property, it is not likely to be used in practice; for, besides its excessive offensiveness, when used in small quantities its power is not sufficient to induce total insensibility, and when a larger dose is employed, it is apt to occasion violent convulsions or death. It appears to affect consciousness more than sensibility; that may be altogether lost while this is not destroyed. Giddiness and nausea ensue, and vomiting or purging are often occasioned, which appear to show that the brain is more affected by it than by some other agents. It appears to act more powerfully upon amphibia than upon warmer blooded animals. Its stimulant property is shown by the instantaneous effects which it produces upon frogs and toads as well as higher animals, even when they appear, and to ordinary impressions are actually, wholly insensible, from the

action of other anæsthetics. If a drop or two of the bisulphuret of carbon be put in any vessel where animals are lying in an anæsthetic condition, they immediately start up and move as though in pain; and, if the dose be not very small, they speedily die. So also its local action produces considerable pain, and is followed by considerable inflammation and swelling.

**CHLORIDE OF CARBON.**—The chloride or protochloride of carbon, in appearance, very strongly resembles chloroform and Dutch liquid, but in smell it is not so pungent or aromatic, and in taste not so sweet; it is, however, remarkable how fluids, having such different compositions as these three have, should have so close a general resemblance in appearance and properties. I am not aware that any one has tried the effects of chloride of carbon besides myself. Dr Snow mentions, in his list of substances, the *bichloride* of carbon, but does not relate any experiments with it, though I presume he must have performed some, and, very probably, under the term “bichloride,” he may mean this fluid, as the term is not used in either Fownes or in the eighth edition of Turner; but in the latter the dichloride is spoken of as a crystalline fibrous substance. It is, therefore, not applicable as an anæsthetic. The chloride is a safe and not unpleasant anæsthetic, in action not dissimilar to chloroform and chloride of olefiant gas, but not, I think, quite so powerful as either of these. The foregoing experiments, however, are, I think, sufficient to justify its trial upon man, and, should it be found to answer, it will be valuable, as being the cheapest of all the fluids which have yet been proposed, at least such as are likely to be used.\*

\* As this sheet was passing through the press, and since the preceding one, containing the experiments with the chloride of carbon, was struck off, I have had reason to suspect that the substance I have been supplied with as chloride of carbon, is not exactly what it ought to be, but that it contains hydrogen. I had expected to have received some chloride of carbon through the kindness of Mr Bullock (this gentleman anticipating the receipt of some, to be made by a new patent process); but as, after waiting some time, it did not arrive, I applied to Messrs Gale and Co., to either prepare or procure me some; they promptly sent me an article as chloride of carbon, which, as I have remarked above, in general character bore considerable resemblance to chloroform and the chloride of olefiant gas; but, inasmuch as these two fluids, though different in composition, specific gravity, and boiling point, in general characters are not unlike each other,—and as, on consulting the fifth edition of *Turner's Chemistry* (for in *Fownes' Manual* and the eighth edition of Turner, 1847, very little is said of the character of chloride of carbon), I found it stated, “This chloride of carbon is a limpid, colourless liquid, very similar in odour and appearance to the oily fluid which chlorine forms with olefiant gas,” as well as the respectability of the parties supplying it,—I had no reason to doubt the purity of the substance. I have earnestly sought from the makers to obtain that exact statement of its composition which would be satisfactory, but hitherto without success. This, however, I will do, and propose to fully investigate the properties of all the compounds of chlorine with carbon, whether alone or in combination with hydrogen. The experiments, with the results, shall appear in the *Journal* as soon as practicable. In the meantime, I would wish these experiments upon the chloride of carbon not to be regarded as altogether

**SULPHURETTED HYDROGEN.**—As the combination of sulphur with carbon certainly possesses some anæsthetic power, I was induced to try if the union of sulphur with hydrogen had corresponding properties. Though I was prepared to find sulphuretted-hydrogen a most potent poison, I must say I was hardly prepared to find its action so intense and instantaneous. It may be that phosphuretted and arsenicated hydrogen are more powerful; but of all the substances, the action of which I have seen, I do not know of one to be compared with it. In No. 246, a dog being exposed to an atmosphere containing only 1 part in 200, for the short space of 25 seconds, was irrecoverably destroyed.\* It certainly is not an irritant poison, and though it probably acts primarily upon the nerves by destroying their action, it also poisons the blood and changes its properties; perhaps, at once, it destroys its vitality. It is curious that water, containing this gas in solution, may be drank in considerable quantity, yet that so very small a proportion inhaled should act so prejudicially. As sulphuretted-hydrogen is one of the gases which is generated by decomposing vegetable and animal matter, emanating from drains and cesspools, &c., may it not, in part at least, account for the depressing action of their exhalations and the fatal influence of malaria?

**BROMOFORM.**—Bromoform, when seen in substance, is a dark-coloured, rather thick-looking fluid, but when seen by transmitted light, it is of a full, rich, dark red-brown colour, a very thin layer having a deep purple tinge. It drops more freely from the bottle than its appearance could indicate. The smell of it in mass is rather agreeable than not, but it is not nearly so penetrating and aromatic as the ethers, chloroform, or Dutch liquid. When a small portion is diffused upon the hands or a cloth, it is

satisfactory and final, lest I should have been deceived in the exact composition of the substance. I should mention that two days ago I gave the fluid sent to me as chloride of carbon to a person under amputation of the leg. The action was pleasant and satisfactory, but a considerable quantity was required, nearly four drachms. It, not improbably, is a combination of chloride of olefiant gas with spirit of wine, and, perhaps, only differs from the fluid sent to me by the same parties as Dutch liquid, in containing less of the chloride, for I am not satisfied that this does not contain some spirit. But, inasmuch as I have made a great number of experiments (and employed it with success during operations) with Dutch liquid, which was prepared by Mr Morson, and also by Mr Bullock, I have no doubt as to the general correctness of the statements with regard to its action, for no one would doubt the purity of an article prepared by either of these gentlemen. The combinations of chlorine, carbon, and hydrogen appear to be exceedingly numerous, and by no means well defined or understood. The exact proportions in which the atoms combine, even with the same process employed, appear, so to speak, to be capricious, or rather to depend upon very slight modifying causes, which as yet are not explained, or apparent to the manufacturer himself.

\* Thenard states, that 1 part in 1500 will kill a bird, 1 in 800 a dog, 1 in 250 a horse.

identical in character with iodoform, without, however, being quite so powerful or permanent. The taste is very pleasant and sweet, without much pungency. It evaporates slowly. The density of the vapour I believe to be high, but do not know it exactly.\* This fluid acts as an agreeable anæsthetic, without producing any irritation whatever, and might probably be successfully employed in practice, were it an article of easy production; but, inasmuch as it does not possess any striking superiority in any respect over other substances of a far cheaper character, its cost will be an effectual bar to its use. I was, however, wishful to ascertain its effects, as forming a link in my chain of reasoning upon the action of these bodies.†

**INJECTION INTO THE VEINS.**—Experiments 251 to 256, show the effect of chloroform, sulphuric, and acetic ether, when thrown directly into the veins, and prove most unequivocally, not only the very rapid action which all these substances possess when so used,—there being but little difference in the quickness with which they act,—but also the very small quantity of each fluid which is sufficient to produce death. The symptoms preceding death, and the appearances presented afterwards, do not differ materially from those caused by inhalation. From these experiments it would appear that, as with inhalation, so with injection into the blood, chloroform is the most powerful and the most fatal, five minims being sufficient to induce unconsciousness and insensibility, but not death; ten minims destroyed, with every symptom of poisoning by hydrocyanic acid, both before and after death; whilst twenty minims almost instantly killed the dog, which was

\* Bromoform is a substance of great rarity, and by no means easy to make. Though mentioned in recent chemical works, the notice is very scanty and imperfect. Indeed, Mr Bullock, to whose politeness I am indebted for what I used, informs me that, having tried the processes mentioned for its production, he found them unavailing, and that he was obliged to institute a series of new experiments before he could obtain sufficient for my purpose. I would here take the opportunity of presenting to Mr Bullock my thanks for his politeness in preparing for me several of the agents used. Feeling certain that the hydriodic ether I had been supplied with was not pure, I requested Mr Bullock to prepare me some, with other substances, expecting, of course, as a stranger, to pay for them. Mr Bullock not only prepared the agents for me, but presented them to me without charge. I am indebted to this gentleman for bromoform, benzole, hydriodic and hydrobromic ethers, bromide of olefant gas, and for some of the chloride of this gas; though part of the Dutch liquid was prepared by Mr Morson, and several ounces, which were sent by Messrs Gale and Co., they have since informed me were obtained from Messrs Heathfield and Burgess.

† I find that Dr Snow states of bromoform,—“It boils at about 184° Fahr., but as its vapour is twice as heavy as that of chloroform, it is, in point of fact, nearly as volatile as that liquid.” “On Narcotism by the Inhalation of Vapours,” *Medical Gazette*, 1848.

In the latter part of this statement, at least, there is, I think, some error; for if an equal quantity of bromoform and chloroform be dropped upon writing paper, the latter will be found to have disappeared within one-fourth of the time of the former.

a large one. Twenty minims of sulphuric ether in one dog did not entirely destroy consciousness, but in another and larger dog, before the whole of thirty minims could be got into the veins, it was very powerfully affected, and shortly was dead. Acetic ether is quite as destructive; indeed, I believe, more so than sulphuric. The effect of injecting hydrocyanic acid into the veins may be seen in experiments No. 142 to No. 152, pages 94 to 98, of the fifteenth volume of the *Transactions*.

**ADMINISTRATION BY THE ALIMENTARY CANAL.**—The experiments under this head, compared with others, are very interesting and important, as they prove most unequivocally that most, if not all, of these agents, when taken into the stomach or any portion of the alimentary canal, produce hardly any anæsthetic effect, acting at first as but little more than local irritants, unless the dose be very large; and even then, not producing so much effect as an equal quantity of spirit of wine. This is a result which, *a priori*, hardly any one would have predicated, and which may assist in throwing some light upon their action, and evidently depends upon some property in the substance itself, inasmuch as we find a considerable difference in this respect amongst them. Some act very violently and most speedily when applied upon the alimentary membrane, but with much less effect when applied by inhalation upon the respiratory membrane, as conia and spirit of wine. Hydrocyanic acid acts with great intensity, administered by either method, while the ethers, chloroform, Dutch oil, bisulphuret of carbon, and especially sulphuretted hydrogen, produce, so to speak, hardly any effect when taken into the stomach, but very decided when inhaled. A comparison of the experiments will show that where scarcely any effect has been produced when swallowed, a like quantity of the same substance when inhaled, even in the same animal, induced, not merely a complete state of anæsthesia, but death. Compare experiment No. 3 with 256, and No. 4 with 258; No. 13 with 259; 261 with 291. When given by means of a sponge, a good portion is blown away by the expired air, so that a less portion actually reaching the lungs produces a much greater effect than when swallowed.

**PROLONGED INHALATION.**—It appeared to be a question of importance to ascertain for what space of time an animal may be kept under the influence of anæsthetic agents, life being safely maintained; whether the mere prolongation of that condition, which for a limited period may be borne with impunity, would be destructive of life; and if so, how long a time would be required to cause death, supposing the largeness of the dose itself were not the immediate cause; what, under such circumstances, are the appearances of the different organs; and whether any great difference exists between the different substances used in the



time during which an animal may be with perfect impunity kept in a state of anæsthesia.

Though these experiments sufficiently prove, that of these agents chloroform is amongst those which are the most speedily fatal, they also demonstrate, that even with it life may be safely preserved, supposing ordinary care be taken not to carry the condition of insensibility too far, for a much longer period than can ever be requisite in surgical operations, or in prudent hands, in obstetric practice. As the *post mortem* appearances shall be referred to hereafter, it is needless to allude to them now.

**REMEDIES.**—Various remedies have been suggested as possessing a controlling power over the effects of too great a dose of ether and chloroform; as yet their advocates can only advance assertion, or arguments based on theoretical grounds, in favour of their respective favourite means. From some of these means upon which most stress has been laid, if my ideas of the action of ether and other anæsthetic agents be correct, little good can be effected; but, nevertheless, it was only right to give them a fair trial. The remedies resorted to are ammonia, cold affusion, agitation, blood-letting, galvanism, and oxygen,—either alone, or in combination. In trying the effects of these, of course it was necessary to endeavour to bring the animal fully under the influence of the anæsthetic, otherwise, by the mere cessation of the action of this, a natural resuscitation would ensue, and, possibly, value be attributed to the means tried to which it is not entitled. Upon these plans we remark in succession.

1. *Ammonia*.—Experiments 283, 4, 5, 6, and 7, will show how far the value attributed to ammonia is just. In the first case it was administered in as large doses, and under as favourable circumstances, as it is ever likely to be in practice, without any advantage whatever; even in two of the cases the anæsthesia was not excessive: while in the other two cases it was given by inhalation, mixed with the agent itself, the effects of which it is assumed to counteract, the result will show the fallacy of the notion; it possesses not the least controlling power over the effects. If, therefore, when given mixed with the agent, ammonia does not prevent its peculiar influence being exerted, it is not likely, when administered after this influence has been developed, to counteract it. Ammonia I regard as altogether valueless; indeed, I am not sure that the activity of chloroform and ethereal oil was not increased in the cases where they were given mixed with ammonia.\*

2. The same experiments and others, distributed in other places,

\* This exactly corresponds with the opinion formed as to the value of ammonia as a remedy against the effects of an over dose of hydrocyanic acid. *Vide* experiments in the fifteenth volume of the *Transactions*.

will show the effect of *cold affusion and agitation*. Both of these appear to me to possess more power than ammonia; indeed, they appear to be the only means which I have tried that exert any influence, but to neither of them am I inclined to attribute any very great virtue. If the case be a bad one, the insensibility very profound, and the collapse suddenly produced, with suspension of the heart's action and of respiration (and, be it observed, where these are not suspended the animal will generally recover, if left perfectly alone), I do not think they will exercise much, if any, influence in preventing death from occurring. When an animal is slowly recovering, the return of consciousness and sensation being prolonged, and as it were halting, then cold affusion and agitation certainly do hasten the process, and the animal recovers both sensibility and motion sooner than when nothing is done; but, so far as I can form an opinion, I am doubtful if this be the case while the anæsthesia is profound, and consequently when the danger is the greatest. I am certain then great violence is not useful.

I do not think the cold douche should be prolonged, nor a large portion of the body be immersed; there is already great depression and coldness. The douche should be merely used to produce a sudden shock; hence I think a small quantity of cold water, suddenly dashed upon the face or chest, the parts being immediately dried, and the process repeated in quick succession a few times, is far more likely to be useful than a prolonged immersion of the body in a large quantity of water. For the same reason the agitation should be sharp and sudden, rather than long and violent. The two may be practised together; and it is a question whether the alternation of heat and cold might not be advantageous. I should be much disposed, between the intervals of dashing cold water upon the head and face, to apply a very hot flannel or cloth upon the chest, and at the same time to agitate the body.

3. *Bleeding*.—The three experiments, Nos. 288, 89, and 90, as well as No. 255, where the anæsthetic was injected directly into the circulating fluid, will show how completely fallacious is the opinion of the usefulness of venesection. As with hydrocyanic acid, the circulation is suspended, hence no blood will flow. This suspension does not arise from engorgement of the heart, owing to some mechanical obstruction to the passage of the blood from one side of the heart to the other, and consequent inability of it to press forward its accumulated blood, but from a primary condition of the heart itself, so that if blood did flow from an open vein, no benefit would ensue; but inasmuch as the *vis a tergo* is almost or altogether annihilated, no blood does flow. Even on a large artery being divided, no blood escaped, none

being propelled into it. The small size of the arterial pulse in an animal in a full state of anæsthesia, will show how feeble is the stream of blood which is propelled into the vessels. As animation returns, and the heart begins to act, blood will escape from the wounded vessel; but this is in consequence of the heart having acquired more power, and not that the heart's acquisition of power is the consequence of the opened blood-vessel, the only effect for which it would be practised. It is not meant to be denied, that in some cases venesection may be useful, as when the heart is weak, by lessening the weight of the column of blood, and thus rendering its work lighter; but, on the other hand, the debilitating effect which the abstraction of blood has, must be considered. As a remedy, blood-letting is valueless, or nearly so, for removing the immediate and threatening condition; it may be useful as a secondary means in some few cases; but as a rule, when the case is urgent, it is useless, for blood will not flow; and where the case is not so urgent, and blood will flow, it also may be useless, or worse, for the case will recover without it.

4. *Galvanism* is a means which many would, *a priori*, be inclined to consider as likely to be efficacious. My experience of its total worthlessness in counteracting the effects of hydrocyanic acid had rendered me rather sceptical as to its utility; nevertheless, I gave it a full and fair trial in the three experiments, Nos. 291, 2, and 3, which, I think, must be regarded as conclusive. It is true the muscles are made most vigorously to contract for a few minutes; but this only appears to excite and exhaust their contractile power more speedily, without in any way restoring their vital action. Galvanism may for a time supply the stimulus of, and usurp the place of volition, but if it stimulates, it does not restore consciousness and sensation. It may be alleged that these animals were so completely brought under the influence of chloroform, that nothing could save them, and that death must, as a necessary consequence, ensue. The result certainly shows that death did ensue; but, considering the comparatively small quantity of chloroform used, the short time it was inhaled, and the manner in which the experiments were conducted, I think we have a right to conclude, that as little was given as could have been, to render the trial a fair one. Many similar animals had recovered from far larger doses, and with less these creatures would have rallied without anything being done; not rallying with the galvanism, we may consider it as powerless as a remedy.

5. *Oxygen* is the remedy which has been more frequently recommended, and its virtues extolled, than perhaps any other; with what truth, the experiments will show. It has been recommended in total ignorance of the action of chloroform and ether, (the two

for which it has been proposed), and hence the misconception of the part which it is to perform.

In recommending several of the so-called remedies and antidotes, one most important circumstance appears to have been altogether overlooked, namely, that, in the cases where their employment is advised, there are no means of administering them. Deglutition is destroyed, consequently fluids cannot easily or safely reach the stomach; and respiration has ceased, or nearly so, hence gaseous matters cannot be inhaled, even were they at hand; for the administering such gases by artificial respiration would be very difficult and imperfect. This is supposing it were certain that they are as valuable as has been supposed, which I believe not to be the case. If oxygen or any other gas is to act as an antidote, it must be by decomposing the chloroform or other agent in the lungs; but as oxygen does not decompose these fluids out of the body, it is not likely to do so within, even did these fluids remain any time in the lungs, which they do not. To act as a remedy, it must be either by directly destroying the action which the poison has occasioned, or by setting up an opposite action which shall counteract it. Now, as we have no evidence that the action of chloroform and allied anæsthetics in any way depends upon the abstraction of oxygen from the blood or tissues, or prevents the exhalation of any noxious matter which the presence of oxygen favours, there is no reason to suppose the presence of this body will counteract or destroy the influence of them.

The inference which appears fairly deducible from reasoning alone, is fully borne out by facts. So far as the experiments with oxygen go, they show it to be altogether useless; indeed, if any conclusion is to be drawn from them, it would rather be that oxygen, instead of being beneficial, is injurious. Experiment 297 shows that oxygen has no power to arrest, retard, or destroy the action of chloroform, even when inhaled with it; while Nos. 294, 5, 6, 8, and 9, show that the inhalation of oxygen, when anæsthesia had been produced by chloroform or sulphuric ether, so far from being beneficial, was injurious; or, to say the least, retarded resuscitation. From the comparative experiments, we see those animals which were uncontrolled, recovered more quickly than those which were placed in an atmosphere of oxygen, and those which were placed in the oxygen appeared to rally immediately on coming into the open air, far more than while they were in the oxygen.

**LOCAL ACTION.**—In the commencement of last year, while engaged with these experiments, the idea was conceived that if these agents acted primarily upon the nerves, then possibly local insensibility might be induced, and thus, perhaps, the necessity

for producing loss of consciousness might at least occasionally be avoided. Some time afterwards, when employing a frog, in putting some chloroform into the jar, a drop or two fell upon it; the pain which it at first apparently produced, and the greater loss of sensation in the part afterwards, seemed to point out these creatures as well adapted for pursuing the subject with; whereupon I made many experiments with frogs, toads, and newts, all of which are particularly susceptible to the local as well as general action of anæsthetics; and subsequently also upon many other creatures, both higher and lower in the scale, as well as upon man, all of which confirmed, in the most undeniable manner, the truth of the opinion. The general fact I mentioned, and exhibited some experiments upon frogs and toads showing it, at the meeting of the Yorkshire Branch of the "Provincial Medical and Surgical Association," held at Leeds, in June 1848. A notice of this communication appeared in the journals and in *Braithwaite's Retrospect*, shortly after which, the fact was confirmed by Dr Simpson, of Edinburgh, who published some very similar experiments, with the same result.

These experiments I regarded at the time, and still do, as more interesting and important in a physiological than a pathological point of view, though they are not devoid of interest and use in this. The phenomena exhibited when a limb or portion of the body is exposed to any of these agents, I have alluded to at the commencement of the account of the local action, and also in giving the details of each experiment, and will not therefore repeat them here. Whether in operative surgery the local action of anæsthetics will be of much use, may be considered as doubtful. In many cases, as, for instance, lithotomy and deep-seated tumours, they could not be used, and in the larger amputations they would not penetrate to a sufficient depth to render the parts insensible throughout, unless applied for a very long time; and it is not unlikely during the long application, the system itself might be affected; besides which, there are many practical difficulties which would impede their use, and which it would not be easy to overcome, as the confining the fluid, without evaporation, to the part wished. In the smaller operations they certainly may be used so as to induce complete local anæsthesia; and if there be any constitutional impediment contra-indicating the internal exhibition, as great susceptibility to their action (as I have known in persons of a hysterical or cataleptic diathesis), there the local action should be preferred; but in other cases it is doubtful, if the longer time required for their application, and the smarting, which, if there be any abrasion of the skin, is great,—and if a wound of any extent, too great to inflict, which is occasioned, be

worth undergoing for the sake of avoiding the inhalation ; unless the part to be operated upon be small, I should say not.

There can, however, be no doubt that the local application of chloroform and other similar agents, in cases of severe pain, is often productive of the greatest comfort and relief. In painful and irritable tumours, the pain may often be alleviated. *Tic doloieux* I have seen disappear like magic, by rubbing the part with it, or saturating a piece of lint and covering it over with oiled silk. Toothach may often be charmed away by a drop put within the decayed tooth. Lumbago and sciatica may be quieted by its use, and in many other cases the local action of chloroform has been very successful in relieving pain, as, indeed, great numbers of medical men, who have since the announcement of its local action tried it with manifest advantage, can attest.

We may now proceed with the endeavour to answer, *seriatim*, the queries proposed at the commencement of this investigation :—

QUESTION I.—*If the property of inducing anæsthesia be confined to the few substances which have hitherto been used, or if there be not a large class of bodies which have in common the property of temporarily suspending consciousness or sensibility, or both, vitality remaining ; and, if this be so, whether these substances are characterized by any similar composition or chemical alliance, and have a common modus operandi upon animal bodies ?*

The experiments related in the foregoing pages prove, most unequivocally, that the power of temporarily suspending consciousness and sensation, more or less completely, is not confined to the few substances which have hitherto almost exclusively been used for the purpose, but that many other substances possess this property in common with sulphuric ether and chloroform. Many in a less degree, some in an equal, a few in a greater, and, probably, there may be other substances in the various compounds of the ether and allied groups, which chemistry is almost daily adding to the list, that may be discovered to possess the power in a still greater degree. To this part of the proposition, an answer may, without hesitation, be returned in the affirmative.

To the second part of the proposition, in the remarks appended to each substance, a partial answer has been given. Those who will take the trouble to refer to them, and to go over the details of the experiments upon which they are founded, will be able to judge for themselves as to how far the conclusion that these substances are so characterized is correct. If those bodies are excluded from our consideration which do not appear, though they may induce insensibility, to produce that condition of the system which is understood by the term anæsthesia, as the protoxide of

nitrogen and sulphuretted-hydrogen, the other substances experimented upon may be arranged into three classes, according to the number of elements which enter into their composition :—

1. The quarternary, into which, besides carbon, hydrogen, and oxygen, one other element enters, as nitric ether; these are very few in number, and not remarkable for anæsthetic power.

2. The ternary compounds, which are far more numerous and powerful. They all agree in having carbon and hydrogen entering into their composition, while the third element is very varied. In the *oleum æthereum*, in aldehyde, in sulphuric and acetic ethers, it is oxygen; in chloric and hydrochloric ethers, in chloroform and chloride of olefant gas, it is chlorine; in hydriodic ether (iodide of ethyle), it is iodine; in hydrobromic ether (bromide of ethyle), bromoform, and bromide of olefant gas, it is bromine; while in hydrocyanic acid, the third element is nitrogen. Moreover, of this third element, even when the same, the relative proportions may vary considerably, the anæsthetic power of the substance remaining very great: for instance, in the atom of chloroform, there are three of chlorine; whereas, in the atom of chloride of olefant gas, there are only two atoms, while the proportion of carbon and hydrogen is much greater in the latter substance than in chloroform.

It is worthy of remark, that, in all these bodies, with one exception, the carbon and hydrogen may be considered as forming a radical base, which unites with the oxygen, chlorine, iodine, and bromine, to constitute a neutral body, or, as we shall presently mention, to act as a base for another combination. In hydrocyanic acid, the base would appear to be formed of carbon and nitrogen, the hydrogen in this playing the part of an acidifier, the resulting compound being the feeblest and most unstable of all acids. This slight acidity can, however, be scarcely considered as a distinguishing character; for we find that conia and other similar vegetable proximate principles, where the same three elements constitute the substance, have a feeble alkaline action, the different character of these bodies depending, probably, rather upon the amount of nitrogen than of the hydrogen.

3. In the third group the substances are all bi-elementary; they are, with three exceptions, hydrocarbons, the exceptions being carbonic oxide, bisulphuret of carbon, and chloride of carbon.

The most simple, obvious, and striking physical properties which the whole of these substances possess in common, is that of being gaseous, or of being so volatile that at the ordinary temperature they are readily converted into vapour.

Although in the first group, that of quarternary compounds, the number of substances is limited, there are several of those which, on ultimate analysis, are found to consist of only three

elements, which most probably, in mode of combination, have a strong analogy, if not identity, with this group. For instance, acetic ether, although only containing hydrogen, carbon, and oxygen, has probably these elements arranged, in the first instance, so as to constitute oxide of ethyle and acetic acid, which, as such, then enter into combination with each other to form acetic ether, as the nitric acid and oxide of ethyle do; this also may, perhaps, be the case with hydrochloric ether. Thus I should take these bodies as illustrations of the correctness of the doctrine which I have before suggested, as to the theory of substitutions. For this also we may refer to the second group, where we find the chlorine may be withdrawn, and iodine or bromine substituted; that is, always provided the substances, if not absolutely isomorphous, are at the least possessed of corresponding forms; otherwise their action will not be similar, and one element cannot then be substituted for another, as in the instance of iodoform, which, so far as the number of atoms goes, is identical with chloroform; yet the one being a fixed, solid, crystalline body, the other a very volatile fluid, they do not possess physiological action, any more than physical properties in common.

If we analyse the composition, and compare the effects of each of the substances in the second group, we find that we may in succession exclude the third element of which each consists, and yet have an effective anæsthetic: thus, in sulphuric ether, we have oxygen, but no chlorine or nitrogen; in chloroform, chloride of carbon, and chloride of olefiant gas, no oxygen or nitrogen, but chlorine instead; in hydriodic ether, iodine; in bromoform and bromide of olefiant gas, bromine is substituted; while in hydrocyanic acid, we have neither oxygen nor chlorine, but nitrogen; so that, by this process alone, we should come to the conclusion, that, however advantageous one or other of these third substances may be in modifying the action of the other two elements (the hydrogen and carbon) of which the agent consists, they cannot be regarded as at all essential to the constitution of an anæsthetic body. This conclusion is rendered perfectly certain, when the constitution and action of the third group of substances is considered. Here most of the substances only consist of hydrogen and carbon, and yet among them are found agents which are possessed of considerable anæsthetic power, as for instance, benzole, common coal gas, and olefiant gas.

In attempting to carry the analysis further, and ascertain if one only of these two elements possesses the property, there is considerable difficulty, in consequence of the impossibility of presenting carbon in an uncombined form; we therefore must adopt another method, that of combining it with other bodies; and, for the sake of comparison, the same must be done with



hydrogen also. Hydrogen, if given alone, does not possess any anæsthetic property whatever, and if given in combination with any other body than carbon, it certainly does not act as an anæsthetic. With oxygen, it is perfectly inactive; with most other elements, it becomes a deadly poison; with chlorine as, hydrochloric acid; with sulphur, phosphorus and arsenic, as sulphuretted hydrogen, phosphuretted hydrogen, and arsenicated hydrogen, these latter being amongst the most violent poisons known. On the other hand, if other combinations of carbon are employed, as the bisulphuret of carbon, carbonic oxide, and chloride of carbon, it is found that these substances do act as anæsthetics; some of them not so safely, pleasantly, or so manageably, it is true, being altered and modified by the other substance with which the carbon is combined; but, nevertheless, still acting as anæsthetics. Hence, I think, we may legitimately arrive at the conclusion, that to constitute an anæsthetic agent, carbon must be present, and that by the combination of it with hydrogen (or perhaps chlorine), we have the basis of the most effective anæsthetic agents. Whether the hydrogen itself plays any important part or not, it is difficult to say with certainty; probably it does, for though, *per se*, it appears to be absolutely negative, yet, as in combination with sulphur, it materially modifies the action of this, as see the different effects of sulphur in combination with hydrogen and with carbon, and the excessive virulence of phosphuretted and arsenicated hydrogen; so we may also suppose it to modify the action of carbon when in combination with it, and thus that it plays an important part.

This view, I am inclined to think, is much strengthened by the fact, that the proportion in which these two substances are combined with each other, appears to materially alter the powers of the compound, whether the two are given alone as a pure hydrocarbon, or in combination with a third body; for the greater the proportion of carbon, the more powerful is the action of the substance. Thus the light carburetted hydrogen does not at all equal in power the heavy gas; and a careful examination of the constitution of the triple substances will show that those which possess the largest proportion of carbon, also exercise the most energetic action. Here, however, very great care must be exercised in discriminating between the effect of the hydrocarbon base, and the proper action of the third element with which it enters into combination; thus nitrogen renders it highly inimical to life, as witness the effect of hydrocyanic acid. The difference in the action of oxygen and chlorine may be seen in the effect of the two corresponding preparations, sulphuric and hydrochloric ether; the one being an oxide of ethyle,  $C_4H_6O$ , the other a chloride of ethyle,  $C_4H_5Cl$ . While the difference made in the action of the

hydrocarbon in combination with the same third element, by an alteration in the proportions of this third element, may be seen in the different effects of the oxide of ethyle (ether), and the hydrate of the oxide (alcohol); the only difference being that in the latter there is an addition of an atom of oxygen and hydrogen; the effect, however, being to reduce the power of this as an anæsthetic almost to zero. As regards chlorine, the action of chloroform and the chloride of olefiant gas may be compared, the former containing three atoms of chlorine, with less carbon and hydrogen than the latter does with two atoms of chlorine; the action of the chloride of olefiant gas being as an anæsthetic quite as, if not more potent, while it is more manageable, in consequence of not being so depressing, arising, I suspect, from the less amount of chlorine itself.

We may, therefore, to recapitulate, conclude:—

1. That no anæsthetic substance has yet been discovered which does not contain *carbon*.

2. That the combination of carbon with hydrogen or chlorine constitutes the best binary agent, and that the greater the proportion of the carbon to the hydrogen, being at least equal to it, the more powerful the agent is; and if we could obtain a pure liquid of hydrocarbon, containing these equal proportions, easy to be volatilized, and of constant composition, it is highly probable that it would constitute a valuable and safe anæsthetic; for, if the carbon be too much in excess, as in benzole, where the proportions are as two of carbon to one of hydrogen, although the compound is a powerful and safe anæsthetic, yet it is neither so manageable nor pleasant as where the proportion of carbon is not so great, symptoms being developed which may reasonably be supposed to arise from the excess of carbon.

3. That hydrogen and carbon may unite, as a radical base, with oxygen, chlorine, iodine, bromine, nitrogen, and perhaps some other substances, to constitute anæsthetic agents, and that these compounds, or at least some of them, may again unite with acids, consisting, perhaps, of the same elements as the base itself, and yet retain, to a certain extent, the same property as the oxide of ethyle, as nitric, chloric, acetic, and hydrochloric ethers do.

4. That of these ternary compounds, those act the best, all things taken into consideration, in which the third element bears the least proportion to the hydrocarbon, especially the carbon, as, for instance, ether and alcohol, perchloride of formyle (chloroform) and hydrochlorate of the chloride of acetylene (Dutch liquid), and of the binary compounds, those in which the atoms are in equal proportion to each other.

5. That substances which are analogous in composition and isomorphous in form, as, in physical and chemical character, so

are they often similar physiological action ; but where they are not isomorphous, their action is not similar, as chloroform and odoform.

6. That substances which are isomeric, or nearly so, but very dissimilar in physical and chemical properties, are also so in physiological ; as witness several of the pure hydrocarbons, which, though very similar in composition, are very different in properties.

7. That should any other combination of elements be discovered which shall possess more valuable and important anæsthetic power than those yet known, its composition will probably be found in accordance with the principles now announced.

Lastly, We may conclude that the action upon the animal economy of all true anæsthetic agents, whether carbons or hydrocarbons, whether simple or combined, is essentially the same in character.

QUESTION II.—*What is the modus operandi of anæsthetic substances ? In other words, upon what structures do they act ? What changes do they produce ? What are their physiological effects ? Is their primary effect local or general ?*

This question it is by no means easy to answer ; probably no reply will be considered by every body as satisfactory and conclusive. In giving the opinion which I do as to the *modus operandi* of anæsthetic substances, I am not insensible to the difficulty and intricacy of the subject. The reply is one that must strictly be founded upon observation. Not only must all the effects during life be carefully watched, but the *post mortem* appearances in the bodies of animals (and, unfortunately, as has too often happened, of persons) destroyed by these agents, must be attentively examined and compared. The number of animals destroyed in the course of this investigation has been large, not only for the purpose of ascertaining the quantity of these agents respectively, which would, as a general rule, destroy life, but also for the purpose of examining the effects produced upon the different organs by them. The details are carefully given in each experiment, and, for the particulars in each case, reference must be made to the report. Another element not to be overlooked in forming an opinion as to the action of these agents, is the varied effect according to the different manner in which they may be presented to the system.

I am inclined to think the action is primarily in all cases, and principally, if not entirely, upon the nerves ; and further, that it is upon the peripheral extremities of these that the direct effect mainly occurs. As this opinion differs materially from that of other gentlemen who have paid much attention to the matter, it

will be necessary to state the reasons which have led to its adoption.

When one of these bodies is applied in substance, or in vapour, to the skin, and evaporation is prevented, the first sensation is that of warmth; smarting and pain soon follow, and with these generally redness and some swelling. After a time the pain ceases, when numbness and loss of sensation ensue. If the application be some time longer continued, most distinctly and unmistakably these proceed along the course of the nerves far beyond the part with which the agent is, or has been, in contact; and in the instance in which I had retained my little finger for some time in a small jar containing twenty minims of Dutch oil, a tingling sensation, with a feeling of numbness, were clearly perceptible along the inner side of the ring finger, but not along the radial side.

When applied upon the tongue, swallowed, or injected into the rectum of animals (see Nos. 256 to 274), the same irritant effect is produced. If chloroform be allowed to come into contact with the lips or face when inhaling it, everybody knows it is apt to blister the thin skin. The effect of those whose sedative power is not very great, when an animal is placed in their vapour, is that of a prolonged irritant upon the conjunctiva and Schneiderian membrane, with some even upon the bronchial membrane. In the case of those agents whose sedative power is considerable, this appears to come on almost immediately; but even with some, as sulphuric ether, for instance, whose anæsthetic power is subsequently very considerable, the first effect is often greatly to irritate the bronchial membrane. These are facts which show these substances act, at any rate in the first instances, upon the nerves, precisely as other substances which are regarded as simple irritants do. This, then, may be regarded as their first action.

The effects upon respiration, the blood, the heart, and circulation are, I consider, only secondary; I do not think the blood is poisoned (with the few exceptions mentioned), or that its organization is materially altered. There are many facts to lead to this opinion. The blood does not lose its power of coagulating, nor is that which is taken from an animal in so complete a state of anæsthesia as to be presently fatal, or even immediately after death has been occasioned, when examined under the microscope, seen to be much, if at all, altered in its characters; consequently neither the fibrin nor the globules can be much changed. Unless the anæsthesia be very profound or prolonged, the blood does not vary much in its colour. That which flows from a wound during an operation is as bright as usual.\* True, after death from these

This at least is the case with chloroform and Dutch liquid, unless the anæsthesia be very profound, or much prolonged. I think, with sulphuric ether, possibly the blood is rendered rather darker than normal, though on my asking two

agents, the blood in the heart and large veins is often found to be of a darker colour than natural; but with many agents, as carburetted hydrogen, the contrary is the case, the blood is decidedly brighter than natural in its hue, and with most, if not all, that which is the most immediately in contact with the substance itself is so; for if there be one appearance more constant than another, it is the crimson or scarlet colour of the lungs. Moreover, even with chloroform and ether, the blood is not invariably dark; and when it is, usually, though not invariably, that within the left side of the heart is not so dark as that within the right. Now, considering that it is the blood in the left side of the heart which has just passed through the capillaries of the lungs, where, having been exposed to the full action of the agent, it will contain more of it than that on the right side; and consequently, if the change in the appearance of the blood depended simply upon the presence of the anæsthetic agent, this change ought to be the greatest on the left side. This, however, is not the case. It might be asked, in how many instances after death by ordinary diseases is the blood found otherwise than altered from that which is regarded as its normal condition, without the cause of death being for a moment attributed to the altered state of the blood itself? That the mere alteration in the colour of the blood is not essentially connected with the anæsthesia is evident from the effects of carbonic oxide, and some at least of the hydrocarbons, when the blood is of a brighter and more scarlet hue than natural. Hence the change in it is not uniform.

The mere suspension of respiration may of itself account for the darker colour of the blood. Some of the substances, if not in excess, may be added to the blood, external to the body, without inducing any change; and, further, we know they not only may be, but actually are absorbed into it, and circulated with it, being either decomposed and carried off as carbonic acid and water,—burnt, or, as with ether, gradually exhaled with the breath for some time afterwards. Moreover, the almost instantaneous manner in which anæsthesia passes off, is altogether incompatible with the idea of the blood being altered materially. Were the effects dependent upon an alteration in the blood, they must necessarily be more permanent; neither should we have the sudden and complete restoration of the faculties, nor the healthy reparative processes usually so well and quickly developed after operations undergone when under the influence of anæsthetics. Long inhalation will sometimes produce a greater alteration in

gentlemen, who had seen as many operations performed upon persons in a state of anæsthesia as most parties have, if they had noticed any change in the appearance of the blood, they said they had not, which they certainly would have done had there been any decided alteration. Several of the experiments show the blood to have been florid in colour.

the character of the blood, as see experiments Nos. 276 and 9; but here, for hours the action of the cerebro-spinal axis was almost suspended; certainly, to some extent, that of the ganglionic system was interfered with; the circulation was most imperfect, and respiration still more inefficiently performed, the influence of which must, in forming an opinion, not be overlooked. I do not deny that these agents are absorbed into the blood. On the contrary, I believe them to be absorbed, but I do not believe the precise amount of their absorption is the direct measure of their effect, nor that their mere presence in the blood is the cause of it. If they be in the blood, of course they will act upon the susceptible lining membrane of the blood-vessels, and be carried to every part of the body; but I see no proof that they act by changing the organization or character of the blood; neither do I think that, to produce a given effect, the blood must be proportionally, according to the respective volatility and solubility of the substances, saturated with each in an equal degree. On the contrary, I believe the effects will, *cæteris paribus*, depend upon the suddenness with which a given quantity is brought into contact with a large nervous surface. Thus a small quantity suddenly thrown upon the bronchial membrane will act far more energetically than a larger presented to it more slowly; and a few drops of chloroform or ether injected into the blood-vessels will induce far different symptoms from those which, according to the calculations of their respective solubility in the blood, they ought to do, if the symptoms merely depended upon the absolute quantity actually in the blood, as compared with what it will dissolve.

The persistence, for days, of the partial loss of sensation and motive power in a finger which has been immersed in an anæsthetic substance (as I have had in my own finger), is altogether opposed to the doctrine of saturation of the blood. During this time, the blood had been changed many scores of times, yet the effect remained confined to the part; and, what is curious, I have sometimes observed it intermittent, returning for four or five evenings (corresponding with the time of immersion), far more perceptibly than in the intermediate periods. The fact of the disposition of the posterior extremities to be more or longer affected than the anterior, has often been noticed, as in Nos. 9, 213, 256, 260, 267, 271, 363. The ready manner in which one limb of an animal may be paralyzed by the local action, and experiments Nos. 318 to 325, in which it is shown that by several, if not all, of these substances, that the entire anterior or posterior half of the newt may be rendered motionless and senseless, the other half of the creature not being so,—all point to the nerves as the parts immediately influenced. So also does the fact so often seen in the local action upon frogs, in which, if one hind leg be

immersed in any of these fluids for a few minutes, the corresponding extremity will become affected before the other portions of the body are ;—while the excitement which an additional dose of the agent, given to an animal already to some extent under the influence of a previous one of the same or another agent, *instantly* produces, is inexplicable on the supposition of mere proportionate saturation of the blood, but perfectly so on that of their acting primarily as irritants upon the nerves. (See experiments No. 275 to 282, especially 278, 312, and 318.)

Another argument, which is of considerable importance against the opinion, that the degree of anæsthesia depends alone upon the degree of saturation of the blood, may be derived from No. 144, in which the pregnant cat was destroyed. Though the animal was evidently fully under the influence of the aldehyde, and, consequently, the blood must have been saturated to its full capacity, yet the fetal hearts continued to pulsate even after the body of the mother had been laid open, and they were removed from her. On the supposition of the blood being saturated, these ought, by having the maternal blood carried to them, to have been paralyzed, if not consentaneously, at least within a short period of her own heart ceasing to act ; whereas, on the supposition of the action of anæsthetic substances being primarily upon the nerves, inasmuch as those of the fœtus are distinct from the mother, and the action of the fetal heart depending upon the integrity of its own nerves, the continued action of it is easily explained.

It may be alleged that either the heart is primarily affected, or that the blood is prevented circulating in the lungs, owing to the congestion of their capillaries and consequent engorgement of the right side of the heart, from the same cause as occurs in drowning and poisoning by carbonic acid. A cursory glance at the condition of the heart in *post mortem* examinations may possibly be thought to support this idea. In most cases, the heart is to some extent filled, and the large veins are also engorged ; but I do not think a more minute and correct examination will support the opinion. Were this so, the right side of the heart exclusively ought to be loaded, the left side to be empty. This is not so ; for, though the left ventricle often contains but little blood, the left auricle generally contains some blood, not unfrequently a great deal. Moreover, though the lungs are frequently scarlet or crimson in colour, this appearance differs materially from the condition of these organs in asphyxia, where the dark carbonized state of the blood is thought to act as an impediment to its passage through the capillaries. Here the blood is in a contrary condition, and though, from the intensity in colour, the lungs are frequently spoken of as greatly congested, it admits of

doubt if they are really much fuller of blood than in a natural condition; and the great collapse so frequently mentioned as occurring in the lungs, when the parietes of the chest are opened, shows there cannot be much distension of their vessels or structure. Besides, if the heart were primarily affected, it ought to be the organ whose action is the first to be suspended; whereas the reverse is the case. The muscles of voluntary motion cease to act first, the movements of the heart continuing long after all action has been suspended or destroyed in them. True, but a feeble stream of blood is propelled, and, in extreme anæsthesia, none whatever; but still the heart moves longer than any other part of the body, (unless, perhaps, the intestinal peristaltic action).

The condition of the heart will be found to vary much, according to the suddenness with which the anæsthesia has been induced. If this has been slow and gradual, there will be comparatively little or no engorgement of the heart; but if it has been rapidly produced, and the examination be made *immediately* after death, the heart will be found to be completely loaded and distended, as, I believe, from the sudden suppression of the nervous energy, allowing the blood to continue to be poured into it without there being any perception of its presence, and hence, without any contractile power of resistance.

Anæsthetic agents appear to act more immediately upon the nerves of voluntary motion and sensation than upon the ganglionic system, hence the functions of the parts to which they are distributed become first suspended or destroyed; and if the action be too intense, then those parts under the more immediate control of the ganglionic system of nerves lose their action. The great length of time an animal may be kept in a state of anæsthesia, with hardly any power in the voluntary muscles, the heart continuing to act with tolerable regularity, confirms this view.

When speaking of the effects of hydrocyanic acid,\* I had occasion to mention the varied conditions of the heart, according to the suddenness of the death, especially the contracted state of the left ventricle, when death had been speedy. To these experiments and observations I would venture to refer. The effects being in many respects very similar to those of anæsthetic agents, they serve mutually to illustrate the action of each other. The fact of the great distension of the right side of the heart, with the frequent contraction of the left ventricle, when death had been suddenly induced, and the examination made very shortly after death, was particularly pointed out; but I was then at a loss satisfactorily to account for the fact. This, I now think, may be explained on general principles. I presume the natural condition

\* Transactions of the Provincial Medical and Surgical Association for 1847, p. 77, *et passim*.



of all the muscles, when first withdrawn from the control of the nerves, is one of contractility (tonicity of the older physiologists), as seen in the ordinary *post mortem* rigidity. Thus when the nervous influence is suddenly and almost instantaneously suspended, the left ventricle remains in a condition of persistent contraction, which it will naturally do much more so than the right, being so much the more muscular of the two, besides not having anything to conduce to its diastolic action. The right side of the heart having ceased, or nearly so, to propel any blood through the lungs, there is no *vis a tergo* pressing upon the left ventricle, whereas the right auricle and ventricle are, to a great degree, under this influence; indeed possibly, though the pressure of the left ventricle through the capillaries is suspended to a greater extent than in their ordinary condition. The fluid blood in the enlarged *venæ cavæ* will press into and tend to dilate the already engorged right side; and, in accordance with an hydrostatic law, that in proportion to the size of the area of the bottom of a column of fluid, so will be the pressure upon the sides of the retaining structure, it is evident this weakened and distended condition of the heart itself materially increases the pressure upon its cavities, at the very time when the feebleness of its walls can afford the least resistance. That this pressure upon the walls of the right side of the heart is too great to be overcome in such a condition of things, may be easily seen by opening the body of an animal in this state, when the right cavities are found in motion, not unfrequently actively so, yet they are unable to propel forwards the blood within them; but if the pulmonary artery be opened, and thus an easy exit be afforded, the blood will flow as the contractions take place. Should the left side happen to contain blood, as it will do if death has not been so very speedy, the same phenomena are seen. If, however, the *post mortem* examination has been delayed for twenty-four or forty-eight hours, the heart will not be found in this condition. The left side may not be found so firmly contracted, and the right side will generally not be greatly distended; at times, not at all. The right side of the heart partaking of the general *post mortem* rigidity (generally also the horizontal position of the body, by removing the perpendicular column of blood in the veins, takes off the pressure upon it, and hence favours the process), gradually contracts and presses out the blood; the degree of which will, among other things, depend upon the quickness with which coagulation takes place, and the firmness of the coagulum, which, in these circumstances, is not usually considerable. The large size of the heart, and the firmness of its contraction in men, who in the full vigour of health have suddenly been deprived of life, has often been noticed, and led some to think there has been an actual

hypertrophy of this organ. This sort of spasmodic action disappears in a comparatively short period after death, so that, if the examination has been delayed for the period just indicated, the left side has lost its great firmness, and the contrast between the two sides is not so remarkable as immediately after death. This view of the difference in the condition, as to dilatation or not in the heart, is confirmed by the examination of that of the kitten destroyed by the prolonged inhalation of benzole, No. 282, in which the change was actually *seen* to take place in the exposed heart; also in 281 and many others, the different condition of the heart immediately, and some hours after death, is noted.

It may be proper here to call attention to the condition of the right side of the heart in two of the animals destroyed by hydriodic ether, in which air was found in it (Nos. 91 and 92). In one of these it appeared possible that the air had entered the axillary vein in making the examination; but of this I am not sure. There was an escape of blood as the vein was opened; immediately air-globules were seen mingled with it, but whether these escaped with the blood, or became mixed with it in rushing into the veins, I cannot say. Probably the air escaped from the vein, as in the quiescent state after death there would not be the same indrawing force,—the dilatation of the heart and the consequent vacuum in the vein,—at work as during life. There is no reason to suppose, in the second instance, the air found its entrance during the *post mortem* examination; if it did, it must have been through a small cutaneous vein, as no large one was opened until the heart had been examined. Air is reported to have been found in the examination of one or two fatal instances from chloroform in the human subject, as in the case at Boulogne; where, however, it has been attributed to the air entering a vein divided during the operation; but, considering the situation and small extent of the incision, this does not appear at all probable.

The condition of the muscles in an animal, after a strong dose of any anæsthetic agent, more especially chloroform and hydrocyanic acid, affords much support to the view now advanced as to their action. There is excessive weakness and loss of control. It is evident the stimulus to their contraction is wanting. The animal is often conscious long before there is any muscular power, or even much sensation (see Nos. 7, 9, 82, 170, 180, 212, 216, 239, and others); indeed, after a moderate dose, or when the dose is not sufficient to induce a complete state of anæsthesia, consciousness remains when there is no power of motion, and but little sensation, as everybody who has inhaled any of these substances knows. The return of muscular power is often accompanied by convulsions, which sometimes are very violent and prolonged, as in Nos. 11, 13, 143, 169, 215, 238, 339, and others.

In No. 323, as the frog recovered from apparent death, after exposure to the vapour of benzole, the movements in its limbs more resembled those induced by galvanism in an animal just dead than anything else. When in some degree restored, it is very irregular, and there is a great want of control over the movements, the animal going staggering and rolling about. When the hand is laid upon the limbs, the muscular fasciculæ often may be felt quivering in a rapid manner, with a sort of vermicular motion. There is always a loss of heat, the vital powers are evidently depressed, the animals are all cold, and for a time appear shrunk. The strong resemblance of these phenomena to those exhibited in an unseasoned man recovering from a very drunken bout, occasioned by a large quantity of ardent spirits, cannot fail to be at once perceived.

The almost total absence of any unnatural appearance in the central nervous mass in animals which have been destroyed by these agents is very remarkable. As a general rule, there is neither effusion nor any considerable congestion,\* being, as I believe, another evidence of the brain not being affected by the changed condition of the blood, or the altered character of the circulation. After death, from the directly narcotic poisons (as, for instance, conia, Nos. 219, 220, 221, or asphyxia), great cerebral congestion is found. This appears, further, to show that the action of these substances is not directly nor primarily upon the nervous centres, but upon the extremities of the nerves inducing a change in their action, which is propagated along their trunks to the spinal marrow, *medulla oblongata*, cerebellum, and hemispheres of the cerebrum; complete anæsthesia being caused when all these portions are involved.† We may, then, conclude:—

\* In several of the animals whose heads were opened, immediately after the vital functions ceased, a serous condition of the brain is noticed. How far this may be abnormal is uncertain; that some, and probably to some extent, a varying quantity of serum is always present, seems certain; but we lack examinations immediately after death, from other causes sufficiently numerous, to feel sure what quantity may be natural and what unnatural.

† I would venture to refer to the following passage from the essay before mentioned, which was written three years ago, before any knowledge was possessed of anæsthetic bodies:—"Though the sensorium is soon affected, as shown by the vertigo, which is one of the earliest symptoms, it is not that portion of the cerebro-spinal axis which appears to be most affected by hydrocyanic acid. At the time I was with Mr A—, I could not divest myself of the feeling that he retained some degree of consciousness, and was, to some extent, cognizant, at least in part, of what was going on about him (and even at first, when I shouted in his ear, that he recognised my voice), at a time when he was unable to give much indication of sensibility. From what I have since seen in many of the animals I have experimented upon, I have little doubt they retain some consciousness and sensibility, when, from the powerlessness of the muscles, they are unable to indicate it. Of course, when the powerlessness is complete, it cannot be proved that either, in any feeble degree, is retained.

"To what extent consciousness is retained it is difficult to say. I am, how-

1. That the action of anæsthetic agents is, immediately and primarily, upon the nerves. That the heart, respiration, circulation, the blood, and the muscles are secondarily affected.

2. That, in the first instance, all these substances act as stimulants, this action being more or less prolonged, but, to some extent, perceptible in all. In the more powerful anæsthetics, as hydrocyanic acid, chloroform, and the chloride of olefiant gas, the stage of stimulation is short and comparatively feeble (unless with the two latter the dose be very small, when it is evident enough), the sedative effect being quickly superinduced. With most of the others it is more prolonged; with ether, it is considerable; with aldehyde, it is still more so; with the bisulphuret of carbon, turpentine, and naphtha, even when inhaled, it is very great, and, when applied locally, intense inflammation only is induced, anæsthesia scarcely being occasioned; while, with alcohol, even when taken internally, unless the quantity be enormous, only the stimulant effect is produced at all.\*

ever, inclined to think it is preserved to a longer period than many are even now disposed to allow. The many cases which have been reported, in which parties have swallowed large doses of hydrocyanic acid, and yet retained consciousness for some time, proves that it may be retained longer than was formerly supposed to be possible; but, even in these, consciousness is thought to be retained only so long as the power of voluntary motion continues. It, however, appears in some cases, where either the dose has not been sufficiently large to prove suddenly fatal, or the susceptibility of the animal to its action but little, that some degree of consciousness may be retained after all power of voluntary motion is lost, and even where there is but little sensibility remaining; at least some of the foregoing experiments seem to indicate this. If this be correct, it would prove that the sensorium is not that part of the nervous system which is most affected by hydrocyanic acid; so also the violent spasmodic action, occurring alternately with paralysis, independent of volition, would point rather to the spinal marrow than to the brain, as being most obnoxious to the action of the poison. \* \* \* \* Reflex action is that which is last destroyed, and first returns; this continues some time after all voluntary motion and sensation have ceased. Touching of the cornea will induce winking of the eyelid when there is nothing to indicate the retention of volition or sensation, and irritating the nostrils of a frog will cause depression of the eye."—*Transactions of the Provincial Medical and Surgical Association for 1847*, vol. xv. page 74, *et seq.*

In this quotation are clearly mentioned the same phenomena which result from a dose of ether. On the very first announcement of the effect of ether, I immediately saw the connection in their action, and would refer to experiments Nos. 87, 87,\* and 87,\*\* page 34, and to the passage at page 89, where, in speaking of the effect of ether, which had been thought to be a *remedy* against the effects of hydrocyanic acid, it is said,—“So far from any benefit resulting from it, *if any effects were produced by it, the action of the acid was increased in power and rapidity.*” As showing how nearly an important discovery may be approached without being actually achieved, the fact was observed and mentioned, but not the practical inferences deduced.

\* Most practitioners will, however, call to mind more than one instance where patients, upon whom a large cloth soaked in turpentine has been applied, have kept it on much longer than was intended. Having borne the stimulant and smarting effect, the sedative has followed, during which they have fallen asleep, an effect which more frequently happens when a large turpentine epithem is used than when a small one; the apparent contradiction of which is readily explained by regarding turpentine as one of the group of substances now under consideration. J

3. That the action is at first local. When these agents in substance or in vapour are applied to a limited portion of the body only, that portion is rendered anæsthetic; but that if the application be more prolonged, or extended over a larger surface, then the effects become general. Thus we may paralyse a part, or the whole of a single leg in the frog and toad; by continuing the application, the corresponding limb becomes involved, and a still farther continuation will affect the entire body of the creature, consciousness, sensation, and motion being all lost. In a large insect or a reptile, we may stupefy the anterior or posterior half of the body with the attached limbs, the other part remaining unaffected. Even with the higher animals, the experiments demonstrate that, in the cat and rabbit, a part of a leg may be locally so affected, as to be altogether insensible to the amputation of it, the general system not differing from its normal condition. In man, a small portion of the surface, or an entire finger, may be made insensible, the feeling of numbness extending along the nervous trunks far beyond the part to which the application is made. Hence we may fairly conclude that the law universally holds good.

4. That, although the action of anæsthetic substances is upon the nerves, and principally upon those in connection with the cerebro-spinal axis, it is not primarily and directly upon the cerebral masses and sensorium, but upon the peripheral expansions of the nerves; that, *cæteris paribus*, the larger the number of these which can be directly and simultaneously acted upon, the greater will be the effect. Hence the *rationale* of the action of these substances when inhaled. When taken in substance into the stomach, they can only come in contact with a comparatively small number of terminal nervous papillæ, whereby is occasioned their stimulant, rather than their anæsthetic effect; but, when inhaled, they come into contact with the whole expanded respiratory surface and the nerves there exposed. Of course, the more concentrated the vapour, the more intense and sudden will be the anæsthetic effect.

QUESTION III.—*Is there any one of these substances which should be selected in preference to, or to the exclusion of, all the others? Is there any one, the temporary effects of which are more complete and certain, the permanent more harmless, and its administration more under our control than those of any of the others, that we should uniformly retain the employment of it, and reject the use of them?*

By far the greater number of the substances which have been tried, even of those which have been shown to be capable of producing anæsthesia, may be dismissed without much comment;

for, though valuable in a scientific point of view, as tending to elucidate the physiological action of these bodies as a class, practically they are not of importance, as they are not likely to be employed while we are in possession of so much better. The substances which appear to possess the greatest power, and the effects of which are the least objectionable, are the oxide of ethyle (sulphuric ether), the gaseous carburetted hydrogens (of which common coal gas is perhaps the best), chloric ether, hydrobromic ether, chloroform, the chloride of olefiant gas, and the chloride of carbon.

*Ether*, which, from being the first substance of this kind introduced, was for some time almost exclusively used, has now almost ceased to be employed. In power, it certainly is much inferior to several of the other bodies; its action is not uniform and certain; more excitement is frequently manifested in its use; it produces much more irritation while being inhaled, and afterwards headach and feverishness are more liable to follow. These are objections so weighty, although in some degree counterbalanced by its general safety and the less liability to collapse supervening, that, unless under the circumstances pointed out in the remarks upon this fluid, and other anæsthetics are not at hand, it will, hereafter, I apprehend, be rarely used.

*Common Coal Gas* is a safe and effective agent. Its cheapness is also an important recommendation. On the other hand, the disagreeableness of its odour, as compared with the pleasantness of the chlorides, and its gaseous form, especially the latter, are serious impediments to its general employment. Under any circumstances, the inhalation of a gas is not of so easy and convenient accomplishment as that of the vapour of an easy evaporable fluid, more complicated apparatus being requisite. In places where the gas is not used for domestic purposes, the inconveniences of transporting and preserving it would be found to be objections not to be despised; but where other agents are not at hand, or from any circumstances are not thought to be altogether applicable, no hesitation need be felt in employing this substance.

*Chloric Ether* is a pleasant substance, both in taste and smell, and, in a very young or feeble person, I should feel disposed to employ it in preference to chloroform; but, in ordinary cases, all other things being equal, inasmuch as a larger quantity of it is necessary than of either of the following, I do not think its use should be urged.

*Hydrobromic Ether* is a safe, pleasant, and effectual anæsthetic. But, inasmuch as it does not possess any such qualities as to render its employment more advantageous than some other substances, the very great cost of it will, unless this can be materially reduced,

entirely prevent its general use. One manufacturer would not prepare it for me under one guinea an ounce.

*Chloroform* is undoubtedly one of the pleasantest and most powerful anæsthetic substances known ; indeed, until I showed the safety of the chloride of olefiant gas, by far the most so. That it is the safest can by no means be maintained with equal certainty ; on the contrary, in safety I believe it to be inferior to all the other substances mentioned just now. Some of the indications for and against its use are alluded to under the heads "chloroform" and "ether," therefore need not here be repeated. There is, however, one unpleasant effect which sometimes follows the administration of chloroform that I have not seen noticed, but of which I have found more than one person complain,—namely, that for long (even months) after its inhalation, they have occasionally experienced the same sensations as they did at the moment of breathing it. A highly respectable professional man (solicitor) has told me that he regrets having inhaled chloroform, which he did many months ago, at Cheltenham, in the extraction of a tooth ; since then he has on three or four occasions suddenly awoke in the night, with precisely the same feelings that he had just before becoming unconscious while inhaling it. He is conscious, but unable to speak or to stir, and feels as though about to die. After a time these sensations pass off, but leave him languid during the ensuing day. He never experienced anything of the kind before, and attributes them entirely to the chloroform. I have heard others say they regret having taken it, feeling nervous and more tremulous than before.

*Chloride of Olefiant Gas.*—This fluid I have now administered so frequently in practice, that I think I am justified in stating the inferences drawn from the experiments upon animals are correct. It appears to be unattended with the troublesome excitement produced by ether, on the one hand, and on the other with less of the tendency to collapse, which is so objectionable in chloroform ; hence if that only true test, experience in general use, should confirm this opinion, it will form a not unimportant addition to the list of anæsthetic agents. In the report upon the action of Dutch liquid, which I made in the *Journal* of the Association, I was misled as to the relative cost of it and chloroform, in consequence of the price of the Dutch liquid sent to me by Messrs Gale and Co. being much less than the price of chloroform when it was first used. I have recently had reason to think, that owing to a want of care, ignorance, or some other cause, an improper method of manufacturing the fluid was employed, and that it contained spirit of wine ; hence the lower cost of its production.

I would, however, here state that it appears not improbable

that a combination of the chloride of olefant gas with spirit, not a mere admixture of the two fluids, but a combination during manufacture, may probably prove, not only a sufficiently cheap, but a valuable anæsthetic; for, since the detection just mentioned, I have made several experiments upon dogs and cats with a mixture of pure Dutch liquid (made by Mr Morson and Mr Bullock) and alcohol, and also of spirits of wine, in various proportions, the results of which I find to correspond with the effects stated to have been produced by that fluid which I now consider to be a combination of the two.

*Chloride of Carbon.*—As stated in note, p. 362, it being not improbable the fluid sent to me by the same parties as above-mentioned, is a chloride of a hydrocarbon, dissolved in spirit, I would not wish the experiments with it to be considered as final and conclusive. Should, however, experiments with a pure chloride of carbon confirm the supposition of its anæsthetic powers, and it be pleasant and safe in use, it will be generally employed, inasmuch as I am informed, that it will shortly be prepared on a large scale, at a very small cost per pound.

QUESTION IV.—*Is there any essential difference in the action of these substances, according to the mode in which they are administered? Do they produce the same effects in whatever way they are carried into the body, or does the effect depend upon the nature of the tissue upon which they are applied? And does the same dose, when applied to the same tissue and in the same manner, invariably produce the same uniform results?*

This question, especially the first clause of it, has been already so fully discussed in the remarks upon the effects produced by the internal administration of these substances, and in the reply to the second question, that but little need now be said. Certainly the same dose, when taken into the stomach, does not (with one exception, hydrocyanic acid) produce the same effect as when inhaled. Though whether this difference depends upon any dissimilarity in the nature of the two mucous tissues is by no means so certain, I am inclined to think not, but much rather upon the greater extent of surface which is brought into contact with the agents when inhaled, and their being in vapour.

With regard to the second part of the question, as to the uniformity of result, it may without hesitation be said that the same dose does not by any means always produce the same result. There is considerable variation in different individuals as to their susceptibility to the influence of these agents, one person or animal being far more easily affected than another. The *very* young (new-born) animal will bear, not only a proportionably, but posi-



tively a larger quantity than the adult, as see Nos. 18, 19, 49, 71, 233, and others; while an older, but not an adult animal, will not bear the quantity which an old one of the same species will, as compare No. 1 with 4, 104 with 108. Moreover, at different times the same animal is far more susceptible to the influence of them than at another, as a comparison of No. 11 with No. 12, and No. 13 with 14, No. 16 with 17, and others which might be referred, will at once show.

Hysterical females are, and experiment 283 would show lively excitable animals also, much more susceptible to the action of these agents than the unexcitable and phlegmatic are. In one instance a lady called upon me while suffering from a paroxysm of *tic douloureux*, anxious to have immediate relief. I rubbed upon the face not more than twenty minims of chloroform with success; but while doing so, another lady who had accompanied her, and stood near, complained of feeling the effect of it. On leaving my house, instead of proceeding to some shops, as intended, the head of the second lady became so confused and giddy, that she was obliged to make the best of her way home again; indeed, she walked so unsteadily, that she afterwards told me she was quite afraid of being supposed to be drunk. She laid down, and slept for several hours afterwards. This lady is one of a family of the most highly hysterical diathesis I ever met with.

The quantity of any anæsthetic which is inhaled in a given time is of the utmost importance;—*cæteris paribus*, the more concentrated the vapour, by far the greater is the danger. Given slowly, the same animal will inhale, without dangerous symptoms, at least twice the quantity that, if inhaled very rapidly, would be not unlikely to cause death, as reference to several of the preceding experiments will show.

**QUESTION V.**—*Are there any symptoms by which we may accurately judge of their effects, and ascertain and control the point, to which the system may, with safety, be brought under their influence?*

This is a most important practical question. Though, without doubt, in every single case we cannot control the effect, yet, as a general rule, it may be affirmed that we can both judge of the effects and control the point at which we wish to arrest the influence of these agents, provided due care be used in administering them. It does occasionally happen that an animal, as No. 283 will show (and I have not any doubt the same occurs in man), is so susceptible to the action of chloroform and other substances, as to be so suddenly and completely brought under the influence of them, even a very small quantity being used, that it

may be found to be impossible to control their action or to prevent a fatal result.

To insure safety, the two principal points to be attended to in the administration of an anæsthetic, are the quantity of the substance used at once, and the temperature at which it is given. Of course, the higher the temperature, the greater the quantity of vapour generated in a given time. It is of the utmost importance to take care that the vapour is not so concentrated as to produce a very sudden impression, or the nervous energy may be so suddenly and completely suspended, that the heart at once ceases to act. I would here remark, that I think it is a very common fault to carry the anæsthesia too far. In order that no sensation of pain should be perceived, it is not necessary to render the patient so utterly powerless as to be altogether unable to stir, as I have often seen done, and as the inexperienced administrator is very apt to do. This is a point of especial importance in the practice of midwifery. I do not here enter upon the question as to the propriety or impropriety of giving chloroform, or any other agent, to the parturient woman, which would lead to a digression not included within the intention of this essay; but I would remark, that, if it be determined to use an anæsthetic agent, that one should be selected which gives rise to the least depression, either temporary or permanent, and that, in all cases, especial care should be taken to give as little as possible. It is rarely, indeed, necessary to produce unconsciousness, which is rather prejudicial than not, by suspending the pains and retarding labour; a dreamy condition of half-conscious obliviousness is, unless there be some considerable manual interference required, all that it is either necessary or desirable to produce. The plunging the woman into a profound state of unconsciousness has, I am convinced, often been productive of the worst and most fatal consequences.

The more simple the apparatus used in inhalation the better; and none, I think, taking all things into consideration, answers so well as a large, moderately fine linen handkerchief, folded first into quarter size, and then into a funnel shape. It is far better than a thick towel; for, while it retains quite sufficient of the fluid, it does not obstruct the free ingress and egress of air so much, consequently the process of respiration is less disturbed. This I have sometimes seen so obstructed by the manner in which a thick towel has been held over the nose and mouth, as to make me afraid of asphyxia being occasioned, and the patient being regularly Burked.\*

\* A very thin, fine sponge, grown in the form of a depressed disk, sufficiently large to cover the nose and mouth, answers the purpose well; indeed, with dogs I found it more convenient than anything else. In the account of the interesting and valuable experiments made by Dr Snow, and which he has been polite enough to send me, since the first sheets of this paper were printed, I find he

The first indication of an animal being pretty well under the influence of the agent, is the deeper, fuller, and more prolonged respiration ; he inhales as though it were pleasant to do so. This is not always observed, though it very frequently is. At first, there is some little excitement, and a disposition to move ; the pulse is rather fuller and quicker,\* the pupils somewhat contracted, and the eyes wandering. There is often incoherent talking. These symptoms speedily pass off, and loss of volition and indisposition, or inability to move, comes on ; the eyes become upturned, the pupils dilated, the pulse smaller, the muscles flaccid, and unconsciousness and insensibility ensue. If the quantity of vapour be now lessened, the respiration becomes more free and natural ; if not, it will be laborious, and presently almost or entirely cease. The lips become closed, and, in expiration, the cheeks may be distended by the air, which, in escaping, may produce a blowing noise. This I regard as a certain sign that the system is so fully under the influence of the anæsthetic as to border upon danger. The distension and blowing noise arise, I think, from the total loss of motion and sensation in the lips and cheeks, which thus become mechanically distended by the force of the expired column of air. At this point or before (for the last symptom is by no means invariably present, the mouth being opened instead of closed), the pupils are widely dilated, the limbs become as flaccid as possible ; there is no reflex action ; the urine or *scæces*, or both, may involuntarily escape, the conjunctiva and lining membrane of the mouth,—often, indeed, the whole countenance,—become pale and bloodless ; the heart may be perceived acting most rapidly, with a short, feeble throb, the pulsation in the arteries being scarcely, if at all, perceptible ; and the respiration becomes either very quick and inefficient, or slow and gasp-

decidedly prefers a jar with a face-piece, as an inhaler, to the simple cloth, thinking the quantity of vapour can be more accurately measured and regulated. If this were so, there can be no doubt of its advantage. This, however, I do not think in practice is really the case, though it may be in theory. Whatever careful measurement may be contrived, much will depend upon the manner in which the patient respire, and the quantity of vapour supplied must be in a great degree at the discretion of the person having the charge of administering it. He must watch the effects carefully, and allow it to be more or less concentrated, according to these. This, I think, is quite as accurately and more easily regulated by the nearness or distance, the close envelopment or openness of the cloth, as it is by the opening or shutting of a valve. Besides, the great convenience of the cloth in every position of the body. Whoever has charge of the giving the vapour should be experienced in so doing, and should not have anything else to attend to. This is quite sufficient for one to watch ; for though general rules and directions may be advantageously given, the application of them in the individual case must be at the discretion of the party who is entrusted with the duty.

\* In some cases of lingering exhausting labour, where the pulse was small and quick, I have seen, immediately the chloride of olefant gas was inhaled, the pulse in a surprising manner to rise in volume and to lessen in frequency. The same occurs with chloroform, but not, so far as I have witnessed, to the same extent.

ing. If this entirely stops for any length of time, even though the heart may be felt still to move, recovery will be very doubtful. In operating, if the surgeon should find the flow of blood to cease, he ought *immediately* to be on his guard, as this may indicate such a condition of anæsthesia as to seriously compromise the heart's action, and, if the inhalation be carried farther, might be fatal.\*

The indications, then, to be attended to in administering anæsthetics, especially the more potent, are :—

Not to employ too much of the agent at one time.

To take care that the temperature be not too high.

To be sure that the air can easily and freely pass into and out of the lungs.

Not in the first instance to allow the vapour in too concentrated a condition to be inhaled, which is easily regulated by not putting the cloth too closely over the face in the first instance ; or, if an apparatus is used, to open the air valve. If there be no excitement, not to increase the strength of the vapour ; but if there be excitement, then, by applying the cloth more closely, or by shutting the valve, to terminate this by more fully and quickly bringing the system under the full influence of the agent, but taking care that so soon as there are indications of unconsciousness and insensibility being produced, in a great degree to withdraw the anæsthetic, and only to give such a diluted vapour as shall just suffice to keep up the condition of insensibility, which, when once produced, may be usually maintained by a very moderate quantity of vapour ; for it must be borne in mind that the *full effect* is not always *instantly* produced as the vapour enters the lungs, nor do the effects always immediately pass off ; indeed, the rapidity with which the effects disappear does not always correspond with their intensity. If these directions be attended to, rarely will death be occasioned by either ether, chloride of olefiant gas, or chloroform, although, as I have before stated, chloroform I consider to be the most dangerous of all ; yet even of this, when due care is exercised, experience has shown the danger not to be very great.

I should not omit to mention, that it is of considerable importance that the stomach should not be distended with food ; if it be, vomiting is much more likely to be caused, and I think a fatal result is more easily induced in such a condition ; at least, in several of the animals in which a small dose destroyed life,

\* Since this was written, the importance of the direction now specified has been painfully shown in a fatal case reported in the *Lancet* for February 25th, 1849, where chloroform was given, without due attention to the cautions above pointed out, to a patient who underwent a small operation. Here the blood ceased to flow before respiration stopped.

the stomach was found distended with food recently taken, as in Nos. 4, 283, 292, and others. I have seen patients during, or immediately after, an operation under the influence of chloroform, vomit food recently taken, in which cases there was considerable depression for some time.

**QUESTION VI.**—*In case of an over-dose, are there any means which can be employed to counteract the effects? Do we possess any antidotes, or are there any remedies? If so, what are they? In what manner do they act, and how should they be employed?*

This question has been in so great a measure answered in the remarks upon the effects of each of the means used as a remedy, that it would only be useless repetition to go over the same ground again. From what has been said, it will be seen that I consider safety is much rather to be found in the avoidance of an over-dose than in seeking for an antidote or remedy against the effects of it. If an antidote of much value exists, it has yet to be discovered. Of the remedies mentioned, no one appears to possess any great power in such a condition of things as would call for its employment. It is true, agitation, cold affusion, a cold stream of air, perhaps electricity or galvanism, and some other stimulants, will expedite the process of rallying where the depression is not very profound; so, also, it is possible that venesection, by lessening the pressure of the column of blood upon the walls of the heart, especially the right, may, when this organ is still acting, facilitate its contraction, and thus assist in restoring the circulation; at the same time, it is by no means certain that, with such a depression as exists in the nervous energy, the abstraction of blood may not in itself tend to increase, or, at any rate, to prolong this condition, precisely as it would do in syncope, to which state it appears not improbable, that induced by anæsthetics, bears some considerable analogy. This idea the symptoms apparently brought on in the cat that had been in the light carburetted hydrogen, No. 167, by holding her in the upright position, would strengthen; and, if the inference be correct, it points out the horizontal as the proper position for a person in such a state to be placed in.

But we have seen, that in the great majority of cases, so long as the heart acts and respiration at all goes on, the mere withdrawal of the anæsthetic is followed by the restoration of the functions; and therefore the question forces itself upon us as to the positive value or not of any of these means. If the heart has ceased to act, no blood will flow on a vein being opened. If respiration has ceased, and the heart does not move, galvanism, as the experiments 291, 292, and 293 show, though it may make the voluntary muscles

violently contract, only does this by the sooner exhausting their power to do so, without in any way restoring the vital functions. Oxygen, of course, if there be no respiration, cannot be taken into the lungs, and the performance of artificial respiration with it would be both difficult and incomplete, even were it at hand, which it would not in the great majority of cases be; while, so far as experiment and chemical action goes, it does not appear that if it were ever so fully inhaled, even in combination with the anæsthetic substance, it would in any manner prevent the development of full anæsthesia, and therefore it cannot be for a moment imagined it would in any way counteract the effects when they are developed. Ammonia we have seen to be useless and inert, whether given mixed with the substance or after its effects were produced. The coldness and depression, which in so marked a degree are present, do not encourage us in the supposition that either cold water or cold air, to any very great extent, would be useful,—a conclusion which the experience of their effects confirms. We are, therefore, reduced to seek from their use just that assistance which shall suffice to produce such a moderate shock, if the nervous system be capable of receiving any, as may cause a reaction and rousing of its powers, by which the heart and respiration may again be set in motion, or rendered more vigorous. This, as I have before said, appears far more likely to be accomplished by the sudden dashing of a small quantity of cold water upon the face and chest, possibly alternating with heat to the latter, or such a moderate stream of air as may be produced by a fan, and a not too violent agitation of the body, than by excessive violence or by entire and long-continued immersion. A moderate interrupted compression of the chest, and pressure upwards of the abdominal viscera, should not be neglected, as tending to change the air in the lungs, and thus to get rid of the vapour there remaining; or, with the same intention, a stream of air might be, not too forcibly, blown through a small tube into the larynx for a minute or two.

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## PART II.

### CRITICAL ANALYSIS.

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- ART. I.—1. *An Inquiry into the Opinions, Ancient and Modern, concerning Life and Organization.* By JOHN BARCLAY, M. D., &c. Edinburgh, 1822. 8vo, pp. 542.
2. *A Review of the Doctrine of a Vital Principle, as maintained by some writers on Physiology. With Observations on the Causes of Physical and Animal Life.* By J. C. PRICHARD, M. D., F. R. S., &c. London, 1829. 8vo, pp. 236.
3. *Hints towards the Formation of a more comprehensive Theory of Life.* By S. T. COLERIDGE. Edited by SETH B. WATSON, M. D. of St John's College, and formerly one of the Physicians to the Hospital at Oxford. London, 1848. Post 8vo, pp. 94.

THE idea of life, though frequently presented to the mind, and familiar by common language, is nevertheless so complicated and so difficult to be distinctly conceived, that it has been more frequently attempted to be explained by what it is not, than by what it is,—in short, rather in negative than in affirmative terms. It has also been attempted to be illustrated by the objects in which it resides, by the phenomena which indicate its presence, and by the actions which, it is understood, are the result of its operation. Life, it is said, is presented by living and organised bodies; and can exist only in connection with them.

The term as thus used is manifestly employed in two different senses;—first, as a particular process or principle in living bodies; and, secondly, as the supposed cause of the actions and processes taking place in living bodies. In order to prevent confusion, various writers have proposed to employ the term *Living Principle*, or *Vital Principle*, to designate the supposed cause of these living actions. The inquiry into the exact nature of this living principle has occupied the attention of many eminent physiologists and other inquirers into the nature of the phenomena peculiar to living bodies. Some, indeed, have either entirely denied its existence, or reasoned as if they denied its existence, chiefly on the ground that it is impossible to demonstrate its existence, and that the effects which are ascribed to it may depend on causes altogether different. On the other hand, it has been almost impossible to reason on the properties, actions, and functions of living bodies, whether vegetable or animal, without

admitting the existence of some principle or quality which is believed to be peculiarly characteristic of living bodies.

It was early observed, that, in all living bodies, there is a peculiar arrangement of constituent parts or structure; and it was naturally inferred that the structure is, in some manner, connected with the movements and actions presented by living bodies. To this structure the name of *Organisation* was given; and though, as we shall have occasion to see, this term has not at all times and by all authorities been employed in this definite sense, yet, upon the whole, the name, as now defined, has been very generally, if not universally, received by all well-informed writers.

In living bodies, therefore, two orders or kinds of phenomena are observed;—one, a peculiar sort of structure; the other, a peculiar species of properties and actions. The latter, it is observed, cannot exist without the former; though, in certain circumstances, it is observed that the structures remain when vital action has ceased in them. The principle called *vital* has been supposed to be the cause or the regulating agent of both.

Physiological speculators have not, in all instances, stated very distinctly the object of investigation in these inquiries, probably from the extreme difficulty of the subject itself. But if we analyze the different modes of procedure by the numerous writers by whom the subject has been professedly or incidentally considered, we find that they may be reduced to one or other of the following questions.—*First*, It may be said, what is the cause of the peculiar arrangement of parts called organization? Is the vital principle or any other agent the cause of this structure? *Secondly*, Whether it be supposed that the cause of the structure is discovered or not, will that structure account for the phenomena and actions observed to take place in living bodies; in other words, are the structure and organization the cause of the actions? In the *third* place, Is there some cause or general agent in addition, which, when the structure is prepared, causes it to enter into those motions which are observed to constitute the actions of living bodies? *Lastly*, Is it possible to refer to the operation of one single principle, all the various properties and actions taking place in living bodies, and the peculiar arrangement of constituent particles by which living bodies are distinguished? To one or other of the four questions now specified may be referred almost all that has been said upon the vital principle as a cause of living movements, and upon life as an attribute of living bodies.

The consideration of this question is so closely connected with various inquiries regarding the nature of the soul, the origin of the body, and the commencement of vital action, that it has led physiological writers into lengthened discussions, mostly metaphysical, upon these points, and upon the question of the connection between the immaterial soul and the material frame. It is



out of our way to go into these parts of the question, which is to be viewed chiefly as a branch of physiology.

Passing over the extravagant and not very intelligible dogmatisms of Paracelsus, the first person who wrote to much purpose on this, after the revival of literature, was Francis Bacon, who, in his *History of Life and Death*, has attempted to compare and contrast these two conditions. The attempt of this great philosopher, however, who rather avoids the question of the principle of life, chiefly shows the difficulties of the subject, and points out to others the path to be pursued and the errors to be avoided.\*

The speculations of Des Cartes are next entitled to attention. He appears to have considered all the phenomena of life, as presented by the animal world in general, as the effects of mechanical structure. All the functions of the animal body,—as digestion, circulation, respiration, and secretion,—he considered as performed by the different organs exactly like mechanical engines. It is probably natural that such a hypothesis should be entertained by the man who inferred the fact of his own existence from the operations of his mind, not from those of his body.

The doctrine that in living bodies there was a separate *anima* or vital Principle, appears first in modern times to have been taught by Harvey, who, after explaining all the phenomena of the incubated egg, states distinctly his inference, that the ovum is the product not of the uterus, but of the *anima*; that in the egg there is a vegetative *anima* or vital principle; and that the various processes of circulation and nutrition observed in the egg can proceed from nothing but an internal vegetative principle. He ascribes, in like manner, to the blood a living or animating principle, by virtue of which that fluid is enabled to form the different organs of the body, to nourish them, and to maintain their growth.

In maintaining this doctrine, Harvey supports his arguments by appealing to the authority of Aristotle among the ancients, and his instructor Fabricius ab Aquapendente among modern authorities.†

Thomas Willis, so distinguished in the history of anatomy and physiology, maintained, in like manner, the existence of a separate or independent principle of life. His views, indeed, which may be gathered from his treatise *De Anima Brutorum*, are not always very consistent; for he speaks, in one passage, of the vital principle as if it was not in the blood but in the *Calidum innatum*, or internal heat; and in others, as if he thought that it did reside in, or circulate with that fluid. This inconsistency is partly occasioned by the circumstance, that, at that time, the valuable discovery of John Mayow was not made fully known or thoroughly

\* The works of Francis Bacon, Baron of Verulam, Viscount St Alban, and Lord Chancellor of England. Vol. iv. *De Dignitate et Augmentis Scientiarum*, Lib. iv., et *Historia Vitae et Mortis*. London, 1778. 4to.

† *Opera Exercitatio Vigesima Septima*. Op. Omnia a Collegio Medicorum. Londinensi, edita 1766. 4to, Londini.

understood. There is strong reason to believe, that, by the *Calidum innatum*, Willis meant the blood after it has been exposed to the air in the lungs, in other words, the red blood in the extremities of the pulmonary veins. It seems almost certain that the physiological speculators of those days applied this term of *Calidum innatum* to the blood which has been aerated in the lungs.

It is unnecessary to do more than refer to the researches of Robert Boyle, and the speculative opinions of Stahl. It is enough to say, that the latter physician was led, by concentrating various opinions, which had been current among his predecessors, to form a theory of the movements and actions of living bodies, in which he ascribed all or the principal phenomena to the influence and operation of the soul upon the body. The doctrine of what were named rather vaguely vital spirits and animal spirits had been previously taught, believed, and generally accredited. This doctrine he rejected entirely, and substituting for the Archæus of Van Helmont the soul as a percipient, governing, and protecting agent, he ascribed to its sleepless and incessant guardianship the well-being of the material frame, its protection from injury of all sorts, and the proper performance of all the functions. Stahl made a correct and careful distinction being living and inert bodies, between organized and unorganized bodies, and maintaining that, in the former, the arrangement of parts is intended for particular purposes, he applied to this arrangement the name of Organism. The soul is his vital principle; yet the textures and organs are endowed with certain qualities which enable them to be acted upon by the soul. Glisson had previously promulgated his doctrine of the irritable power; and Stahl had evidently in view, in the admission of the properties now mentioned, that of irritability, contractility, and similar qualities.

Hippocrates, or one of the writers who passes under this name, had imagined, in the animal body, a principle to which he gave the name of Impeller or impelling Agent; (*Ενοργανον*); and to the agency of this principle he ascribed all those actions and phenomena, by which animal bodies are distinguished, especially sensation, motion, the property of being acted on by stimuli, or what was afterwards named irritability, and, in short, all the involuntary and automatic motions of the animal body. The operation of this impelling principle was admitted by Hermann Boerhaave; and in 1745, his nephew Abraham Kaau Boerhaave published on its nature and effects a learned essay, in which he is certainly the expositor of the opinions as much of his uncle as of himself.\* He gives in this treatise an account of the mental faculties and their mode of operation, an exposition of the hypothesis of gene-

\* *Impetum Faciens dictum Hippocrati per corpus consentiens observationibus et experimentis passim confirmatum. Auctore Abrahamo Kaau Boerhaave, Lug. Batavorum, 1745. Cap. iii. and iv.*

ration and conception by germs, and an estimate of the influence of the spermatic animalcules. He then considers different opinions on the mode in which the universe was created, and inquires into the creation of living beings, which he ascribes to the impelling principle, which is action neither of mind nor of body, but something peculiar and yet created. This impelling principle is nothing but the vital principle of other physiologists, and it is either the source of irritability, contractility, the sentient principle, and similar abstractions, or it is the same as these.

The treatise of Kaau Boerhaave contains many valuable illustrations of the nature and effects of the vital principle; and if that term were substituted for the one which he employs to translate the Greek *Ενοργανον*, the book may be read as one giving a just view of the subject, so far as it is explicable and intelligible.

Stahl had in Germany many disciples, by whom his doctrines and hypothesis of Psychodynamicism were more or less vigorously promulgated. In Great Britain they were espoused and chiefly made known by Richard Mead, William Porterfield, in some degree by George Cheyne, and still more explicitly by Francis Nicholls, the anatomical teacher at St George's Hospital in London. But these doctrines received their complete development and their most ample and ingenious degree of elucidation from the labours of Robert Whytt. The lucid reasonings of Whytt, and his copious and pointed practical applications, have procured for this theory a degree of credit, which it never of itself could have acquired; and it must be regarded as a remarkable proof of the great genius of Whytt, that, though the original idea of Stahl is now mentioned only as matter of scientific history, the commentaries which that author produced, are undiminished in value, and will continue to be studied with interest so long as physiology is cultivated.

All the involuntary and instinctive motions of the human and animal body,—as the motions of the heart, the contractions of muscles in general, the motions of the stomach and intestinal tube, the first movements of respiration, the act of sucking by new-born animals,—Robert Whytt ascribes to the agency of the mind or **SENTIENT PRINCIPLE**. He allows that certain sensations take place, and that these determine the sentient principle to put certain muscles and organs in motion. In torpid and hybernating animals, this sentient principle is in a state of inertia or perhaps sleep; and, on the returning heat of the sun and atmosphere, there is communicated to the blood and fluids a new motion, which excites a peculiar sensation; and this rouses to action, as it were, the sentient principle, which then causes the contraction of the inspiratory muscles, and alternately the expiratory, and thus gives rise to the act of respiration.

We cannot but acknowledge, argues Whytt, that the author

of Nature has animated all the muscles and fibres of animals with an active SENTIENT PRINCIPLE, united to their bodies ; and that to the agency of this principle are owing the contractions of stimulated muscles. If it be imagined, he continues, that He has given to animal fibres a power of sensation and of generating motion, without superadding or uniting to them an active PRINCIPLE, as the SUBJECT and CAUSE of these, he presumes to say, that a supposition of this kind ought not to be admitted ; since, to suppose that matter may, of itself, by any modification of its parts, be rendered capable of sensation or of generating motion, seems to be as unreasonable as to ascribe to it a power of thinking. Matter, he continues, as far as we can judge by its known properties, appears to be incapable either of sensation or of thought ; and the whole appearances of the merely material world, show that it acts invariably according to laws prescribed to it, and without any feeling, inclination, or choice of its own ; nor is there anything resembling will, self-determination, or real active power, in the most refined and subtle parts of matter, more than in the most gross and sluggish.

If, then, he concludes, the effect of *stimuli* upon the muscular fibres of animals cannot be deduced from any property or powers belonging to them, as mere material organs, it remains that they are owing to an active SENTIENT PRINCIPLE animating those fibres. After another train of reasoning, he states the general result in the following manner :—"Upon the whole, as Nature never multiplies causes in vain, it seems unnecessary, in accounting for the motions of the muscles of animals from *stimuli*, to have recourse to any hidden property of their fibres, peculiar activity of the nervous fluid, or other unknown cause, when they are so easily and naturally explained, from the power and agency of a known sentient PRINCIPLE."

It must not be imagined, nevertheless, that Robert Whytt went so far as Stahl in admitting the influence of the soul as a rational agent over the body. Against this extreme he was most anxious to guard himself and his readers ; and he enters into a discussion, the object of which is to point out the exact limits of this influence of the sentient PRINCIPLE. He allows that the notions of Stahl and his immediate followers, with regard to the manner in which the mind regulates all the actions of the body, are extravagant ; and he takes occasion to specify the errors into which Francis Nicholls had fallen by adopting these views in their extreme degree. He rejects, accordingly, the idea that the soul at first creates the body, that it afterwards exerts a constant care over it, heals its diseases, restores its actions when disordered, and if it perceive that the corporeal frame is no longer capable of reparation and maintenance in a comfortable state, finally abandons the

dilapidated and ruinous mansion. All this, he conceives, is unfounded and incredible.

Many philosophers had, in imitation of some of the ancients, admitted the existence of two distinct principles in man ;—one, the *anima* or soul, (Ψυχή) *Psyche*, the principle of life and sense influencing the vital motions ; the other, the *anima* or mind, *Nous*, the seat of reason and intelligence. The *anima*, or vital and sentient soul, they imagined man possesses in common with brutes ; the *animus*, *mens* or *Nous*, which is of a more exalted nature, is proper to rational beings alone.

With some modern materialists, it was a favourite doctrine to represent the *anima* to be no other than a subtile kind of matter lodged chiefly in the brain and nerves, and circulating with the grosser fluids. Such vital spirits or subtile matter, Whytt argues, can no more be acknowledged to be the vital principle or source of animal life, than the blood from which they are derived ; and with greatly less reason can this material *anima* be supposed to be endowed with sense, since of itself, and unactuated by any higher principle, it is equally as incapable of sense or perception, pleasure or pain, as it is of self-motion.

All this doctrine, in short, Whytt regards as totally unfounded, and he maintains that the *anima* and *animus*, or the sentient and rational soul, are only one and the same principle acting in different capacities.\*

The essay of Robert Whytt was published in 1751 ; and in no long time, the views and reasonings contained in it attracted considerable attention and excited great interest. Continuity prevented us from noticing, what chronological order would have required, that a few years previously, namely, in 1746 and 1747, another writer published various dissertations, which certainly contain some rather aspiring pieces of speculation, but were sadly deficient in all the most important and useful requisites of giving intelligible and new information. M. Quesnay, the same who, we believe, afterwards became celebrated as a writer on Political Economy, published on the Animal Economy three essays, in which he undertook to rectify various prevalent mistakes, to supply various wants much felt, to raise the character of physicians, and to invest the practice of medicine with a more philosophical character than it had previously possessed.

M. Quesnay, not doubting the existence of a vital principle, states that it has been at all times allowed, that this principle resides in the nerves, and that this is proved by experience ; that this principle consists in a most subtile and active fluid, invisible nevertheless, which does not act by vibration, as supposed by some, and the nature of which is, in other respects, unknown, except by

\* Essay on the Vital and Involuntary Motions of Animals. Edin. 1751, 1763, and Collected Works, Edin. 1768.

its effects. He conjectures that it is of the nature of the Newtonian ether, which, he thinks, is retained within the nerves. This vital principle animates the parts, and puts them in motion. It is the first material agent which gives life, motion, and sensibility to the parts of the animal body.

Various Christian philosophers have distinguished, he states, three species of souls, or substantial forms, in living bodies ;—one, a material soul confined to plants, and denominated the *Vegetative* soul ; another, also material, called the *Sensitive* soul, and which, with a vegetative soul, is given to brute animals ; and a third, the *Rational* soul, which, as the source of the more exalted faculties of thinking, reasoning, and comparing objects, is, with the vegetative and sentient soul, granted as the peculiar characteristic of man.

Some modern philosophers, especially the followers of Des Cartes, reduce all these three souls to one, which is at once Sentient and Rational, which they allow to man only, recognising in brute animals no sentient soul, but merely the faculty of seeing, hearing, knowing, distinguishing, having appetites and passions, which, however, are all merely mechanical. This question, however, is not decided among philosophers ; and he thinks it is an unprofitable speculation.

M. Quesnay then enters into the detailed consideration of the faculties supposed to belong to these three different kinds of souls, and makes many interesting observations tending to illustrate their peculiar characters and differences. In psychology he is a disciple of Malbranche ; in physiology, he seems to adopt the views of Boerhaave.

M. Quesnay has the merit of anticipating Robert Whytt in recognising the impropriety of the extreme views of Stahl upon the power of the soul in forming the body. Physiologists, he argues, who entertain this opinion, know little of the limits of the knowledge and power of the soul. It is manifest that it does not even know the parts of the human body, and that it is altogether ignorant of their shape. The soul becomes acquainted with these parts only by studying them in a body different from that which it occupies ; and, after long researches, the knowledge which the soul in this manner acquires, is very superficial and very imperfect. The soul, void of its corporeal organs, can neither move matter nor arrange it, to form the body which it is to animate. It does not even, without education, know the properties of matter. Without its corporeal organs, the soul obtains no ideas, no perceptions of substance. It is unable to move an arm or a leg. How can it construct any of the organs, or repair them if injured ?

Life, which depends on the continual reproduction of the fluids which fill the vessels of the animal body, appears under two forms, which must be carefully distinguished ; vegetative life and

animal life. Vegetative life, or the life of the body, consists in those organic movements which constitute the digestion of food, the action of the heart, arteries, and other vessels; the circulation of the blood, the formation of the fluids, and all the other operations of the living body. They are independent of our sensations and of our thoughts. They are performed whether we are sleeping or awake, and are not under the influence of our sentient and reasonable soul.

Animal life, on the other hand, depends on the body and soul together, and it consists only in the sensations which the living body causes in the soul, and in the power which the soul possesses of determining certain movements of the members of the body, to make them move the body in different directions. When it is said, that the soul animates the body, all that ought to be understood is, that it causes a living body to act, and not that it animates this body. Thus intelligence, knowledge, power, the desires of the soul, do not extend to the mechanism, to the movements, and to the actions which give life to the body and maintain it in life.

This is the reason why the ancient philosophers recognised the necessity of some other PRINCIPLE for constructing the parts of the living body, and to direct those movements in which consist life and all the operations of the animal economy. This principle they denominated the VEGETATIVE SOUL,—a principle which must be infinitely more intelligent, more knowing, more powerful than the RATIONAL SOUL. These philosophers, however, erred greatly in confounding this creative Intelligence with Matter. Some considered it as a part and a dependent on the substance of the body. It is neither. It ought to be referred to the Supreme Intelligence; and the existence of this intelligence is recognised by these operations on matter.\*

Animal life consists in the sensations and perceptions which affect the sentient being. When the exercise of the faculties which convey these sensations is entirely interrupted or suspended, this being is, as it were, reduced to nothing. It is brought to a state of negation. The happiness of animal life consists in agreeable sensations and perceptions, and its unhappiness in unpleasant sensations and perceptions. During sleep the action of Animal Life is, as it were, suspended; and all the operations of the body are reduced to those of Vegetable Life.

M. Quesnay gives views of the mental faculties and the formation of our knowledge similar to those taught by Locke and afterwards by Reid. His account of the moral sentiments and passions is interesting, and shows that he was an active and correct thinker.†

\* *Essai Physique sur l'Economie Animale*. Par M. Quesnay. Seconde Edition. A Paris, 1747. Tome Troisieme, chap. xv. p. 126.

† *Ibid.* chap. xvii, xviii, xix.

It is manifest that Quesnay preceded Bichat in forming the distinction between animal and organic life. The vegetative life of Quesnay is the organic life of Bichat. In many other points, especially the connection of the moral affections with the organs of organic life, and the influence, favourable or hurtful, upon these organs, he has also anticipated the great physiologist of the modern French school. In his delineation of the functions which are under the dominion of animal life, he deprives Bichat of all merit of originality.

The researches and labours of Haller in all the departments of physiology are too well known to require being mentioned here. His controversy with Whytt shows that with the doctrines of Stahl he was not satisfied. Though he nowhere formally announces his opinion of the existence and nature of a peculiar principle of life, yet, as he ascribed to muscular fibre the irritable property, and distinguished it from the nervous power, and as in generation he adopted the hypothesis of evolution or development by the generative process, he is usually classed among those who regard the phenomena of life as the result of organisation.

About this time arose another inquirer, who appears, in a spirit of judicious eclecticism and analysis, to have attempted to give correct and consistent views on the nature of the principle of life. This was Matthew Van Geuns of Groningen, who published, on the 14th of June 1758, a physiological disquisition on the principle of life in the animal body.\* This author, who was a pupil of Van Doeveren, Boerhaave, Albinus, and Gaubius, has examined with judgment and learning the question of the cause of life and vital phenomena. The actual presence and exercise, says Van Geuns, of these causes and forces which constitute the animal nature of the body, forms what I call life. It is manifest that to this many conditions of structure, property, and action are requisite. Life is most perfect when the composition of the parts and the structure is that which is proper, and when all its forces act with proper energy, in the right direction, and steadily. This constitutes that complete arrangement of the economy called health. But, as some defect may cling to the structure and mechanism, or may take place in the aberration and deficiency of the forces, then is formed the idea of the morbid state. If these defects be carried to so great a degree that the economy is deprived of the service of several parts which have become useless, while it still exercises certain functions peculiar to itself, life is indeed continued, and the economy lives. But if it be so mutilated and

\* *Disquisitio Physiologica de eo quod Vitam Constituit in Corpore Animalium* Quam Praeside G. Van Doeveren, M. D. Publice Examinandum proposuit. Auctore Matthaeus Van Geuns, Groninganus. Groningæ, 14 Junii 1758. Apud Sandifort Thesauri Volumen tertium, L. Bat. 1778.



enfeebled that it is no longer able to perform the most necessary actions of the economy, then life ceases, and the body, with this extinction of all its living forces, is dead.

Van Geuns then examines in what actions life consists, and considers the dependence of vital phenomena on respiration, on circulation, and on the action of the brain and nervous system. He follows the usual custom of distinguishing the actions of living bodies into vital, natural, and animal. He eventually concludes that the living body is such a machine, or combination of machines, that it restores loss of parts, and repairs waste of strength, and therefore possesses within itself conservative powers. There is a sort of circle of functions in the animal body. Yet it must not be imagined to be either a mere machine, or an instrument of perpetual motion. Irritations are not the true causes, but only the occasions of contraction. Irritability is an important and wonderful property of the organs of animal bodies. But it is not sufficient for life that these organs be endowed with this property. There must be besides in these parts of the animal bodies such a composition of fluids, that these parts may apply to their particular purposes, and distribute to their proper situations the fluids necessary for maintaining the growth and strength of the frame. Life therefore seems to depend on two circumstances,—irritability, and the constant supply of materials of nutrition. There are also fibres of perception and fibres of motion.

Apparently Van Geuns was conscious that this was not a very clear explanation of the nature of the vital principle; and his speculations and reflections must have led him to infer, that, as of many abstract ideas it is requisite to form conceptions by having recourse to negatives, so it was impossible to form adequate ideas of life without considering what was the nature and character of death. Whether this was his reason or not, certain it is, that, three years subsequently, he published, on the occasion of taking the doctor's degree, a pathological dissertation on Death, and the causes of death.

Though the proximate or efficient cause of death is always the same, yet death, he maintains, is produced in three modes—Mechanical Death, Dynamical Death, and Death in the Mixed Mode.

Mechanical death is that which is the result of injury done to some of the vital organs, and embraces the whole order of death, by organic changes, wounds, injuries, and similar means. Dynamical death is that, in which no perceptible change has taken place in any of the organs. Death in consequence of vices of mixture embraces death by putrefaction, gangrene, poison, epidemic diseases, as fevers of all sorts, plague, dysentery; poisoning by bad or spoiled articles of food; by the bites and wounds of poisonous and diseased animals; by excessive cold; by lightning; and by spontaneous combustion.

It rather takes away from the unity of his arrangement that he afterwards refers to the head of dynamic death, death by poisons, by vapours, and contagions ; by thunder and electricity ; and notices especially among that by poisons, death by monkshood, water hemlock, moonseed, hellebore, the bitter almond, and cherry-laurel water, and similar agents, and even the vapour of the *grotto del cane*.\*

In these two dissertations, Matthew Van Geuns evidently anticipated Bichat in his celebrated treatise on life and death. It is true the Hollander is slow, painstaking, a little heavy and dull in his mode of making statements and reasonings, and, after the whole is concluded, the reader feels that he is little wiser than before. Yet this is probably the evil of the subject, which is avowedly difficult and complicated ; and, while it must be allowed that the learned author has thrown little new light on the nature of the vital principle, it is also not to be forgotten that he undertook, in the middle of the eighteenth century, the solution of the problem which Bichat may have elucidated, but did not determine in the nineteenth century, and which seems as remote as ever from final solution.

About the same time, namely, in 1757, or rather previous to that year, which was the era of their publication, William Battie, a learned fellow of the College of Physicians in London, delivered before that body a series of twenty-four discourses on the animal principles. It is stated by Sprengel, in his history of medicine, that William Battie agreed with Haller as to the explanation of the muscular force. In some respects, this is correct. Battie, however, was a disciple rather of Boerhaave and Albinus than Haller, and professes more than once that he wishes to be understood to follow no one particularly, but to judge for himself. After relating the not very gentle experiment of Albinus, in which that anatomist cut off the head of a turkey-cock as he was walking, without the animal being aware, and in which he continued to strut on and move his wings, while the cut muscles of the neck were retracted and alternately relaxed,—he draws two conclusions ; —1st, that every muscle contracts, though the contractile power or corporeal cause of motion derived from the brain, is entirely withdrawn ; and, 2d, that any muscle cut off from the body, contracts in consequence more forcibly than usual, because that muscle is now at freedom, being no longer under the control of the nervous system, and because all impediment to its activity is withdrawn.

He adds, that he does not thence infer that the nervous system contributes in no way to the action of muscular movement ; and that he thinks that the use of the brain is rather to keep within

\* *Dissertatio Pathologica de Morte Corporea et Causis Moriendi. Quam Pro Gradu Doctoratus publice defendit Matthæus Van Geuns ; Groninganus. Lugduni Batavorum, 14 Junii 1761.*

the bounds required by health, the muscular action, the force of which would otherwise be enormous. He is inclined, with Stahl and Willis, to admit the existence of an *Anima Brutorum*, or GENIUS of the brain, which, like a faithful guardian, takes care that the bodily machine may sustain no injury.\*

Dr Battie subsequently reasons, not very distinctly, on the question of an independent animating power or corporeal spirit, evidently understanding a principle of life.† But he appears to have thought that, if he expressed himself in more decided language, he might have incurred a charge of materialism. This was a peril not to be lightly incurred by one who was a member of one of the Universities.

The controversy between Haller and Whytt on the influence of the *vis insita*, and that of the nervous system over the muscles, produced many speculators and writers on the nature of the living power, and the exact nature and seat of the principle of life. These it is impossible even to enumerate at present. It would be wrong, however, to omit so great an authority as Cullen, who, though neither a professed anatomist nor an experimental physiologist, united a rare degree of sagacity and acuteness in analyzing the nature of vital phenomena, with the greatest ingenuity in applying them to explain the phenomena of diseases and employ remedies for their treatment. This physician supposed LIFE, so far as it is corporeal, to consist in the excitement of the nervous system, and especially of the brain, which unites the different parts, and forms them into a whole. But as certain other functions of the body are necessary to the support of this excitement, we thence learn, he adds, that the causes of death may be of two kinds;—one that acts directly on the nervous system, destroying its excitement; and another that indirectly produces the same effect by destroying the organs and functions necessary to its support.‡

Gaubius had previously applied to the contractile power of muscular fibre the name VITAL POWER. This Cullen named INHERENT POWER, supposing, we presume, that the vital power was a more elevated property of the living body.

The physiological view of the doctrine of Life and the Vital Principle, as it was taught at the Edinburgh school, was explained with much ability in 1782 by John Theodore Van Der Kemp, a Dutch student from Rotterdam, who made it the subject of his Inaugural Dissertation. Life Van Der Kemp defines to be the FACULTY OF EXERCISING THOSE ACTIONS WHICH BELONG TO

\* De Principiis Animalibus Exercitationes Viginti Quatuor in Theatro Collegii Medicorum Londinensium Habitaæ. A Gulielmo Battie, M. D., C. Ejusdem S. Londini, 1757. 4to. Exercitatio Sexta, p. 76 et 80.

† Exercitatio Octava, p. 117.

‡ Institutions of Medicine. Physiology, cxxxvi. Thomson edition, Vol. i. p. 135. 1827.

THE UNITY OF A SYSTEM AIMING AT UNITY. He thinks that there is in man a threefold principle of life ;—first, the body, or that material part which is divisible by finite power ; secondly, the *anima* or soul, which involves in its essence the principle of life, and exists with the material body ; and, thirdly, the *vehicle* or seat of the soul, which is that corporeal matter, which unites with the soul into a compound multiform substance, and which cannot be dissolved by the combined forces of the whole universe. The author subsequently defines the life of the human body to be the FACULTY OF PERFORMING THOSE ACTIONS WHICH THE UNITY OF MAN REQUIRES, or that mode of the body in which, along with the soul and its vehicle, it constitutes the living mass.

This, it must be allowed, is not very clear. Yet it is probably as intelligible as the subject itself admits. Van der Kemp is something of an eclectic, and, while he gives attention to the opinions of previous writers, he seems to adopt the views of Whytt, at least, if not of Stahl. He says that all must allow, that the soul is the efficient cause of the life of the body, so that the principle of life is by all considered to be the character of the soul. Again, he proves that the life of the body consists in the determinate composition of corporeal parts ; and, if these two propositions be united, he infers that he proves, that the soul is that cause or agent, by the power of which the parts of the body are conjoined in a determinate manner.\*

The experimental inquiry of Edmund Goodwyn, which was published in 1788, tended to elucidate much the opinions of physiologists on the conditions essential to life, and may be regarded as the source of those showy productions which afterwards appeared in Paris from the successive hands of Bichat, Le Gallois, and Nysten.

Vitality, says Blumenbach, belongs to that class of things which are more easily distinguished and understood when present, than defined in words ; and, indeed, by definition, they are in general rather rendered obscure than illustrated. Vitality is, however, manifested by its effects, which must be referred to peculiar FORCES, which are therefore named vital (*vires vitales*), because upon these depend the actions of the organic living body. The vital force (*vis vitalis*), he adds, is, as it were, the mainspring of physiology, and has been at all periods recognised by physiologists, though under most different denominations. At one time, it has been named the IMPELLING AGENT, *Ερωγυων* ; at another, the INNATE HEAT, (*calidum innatum*) ; the ARCHÆUS, the VITAL SPIRIT, the

\* *Dissertatio Medica Exhibens Cogitationes Physiologicas de Vita et Vivificatione Materiae Humanum Corpus Constituentis, &c.* ; auctore Joanne Theod. Van der Kemp, Rotterdam-Batavo. Edinburgh, 1782. 8vo. ix. l. and passim.

This dissertation is dedicated to Edmund Goodwyn, whose dissertation on the Connection of life with Respiration acquired much more celebrity.

SOUL OF BRUTE ANIMALS, the RULER of the NERVOUS SYSTEM, the SENTIENT PRINCIPLE, TONIC VITAL ATTRACTION ; and at different periods and by different persons differently.\*

Most just and important is this remark of Blumenbach. The number of names employed to designate the same object, if they prove anything, must be allowed to show, that there is some general principle on which vital phenomena and all vital actions depend, if this principle could be rightly explained. It seems, nevertheless, entirely beyond the reach of the human faculties, if we consider, how many attempts have been made to explain its nature, and that it is still not understood.

In 1787, the same author was led to consider the subject more directly in adverting to the question of the vital power of the blood. This, it is known, was maintained by Harvey and John Hunter. Blumenbach allows a vital power to the living solid, that is, nerve and muscle, and cellular tissue ; to the first, sensibility or the power of receiving impressions ; to the second, irritability ; and to the third, contractility. To the blood, however, he refuses the property of vital power, and maintains that what is called life of the blood is merely the life of the blood-vessels and other solids to which the blood is distributed. To none of the fluids of the animal body, he contends, can we justly ascribe a vital power, unless perhaps the seminal liquor of the two sexes ; and the proof of this is found in the new life caused by the union of these fluids, in contributing to the formation of the foetus.†

Previous to the close of last century, a French writer, named J. B. Fray,‡ attached to the Commissariat Department, published an essay on the origin of organised and inorganic bodies, and on some phenomena of animal and vegetable physiology ; in which he gave utterance to some very extravagant opinions regarding the origin of the material world and of animal bodies. He maintains that the substances proceeding from the sun in the form of light, uniting with those proceeding from the same source in the form of heat, electricity, magnetic fluid, and galvanic fluid, are constantly forming combinations, and producing not only mineral substances, but certain organic globules or atoms, endowed with life and voluntary motion ; and that these globules, combining under different circumstances, form all the genera and species in the Animal, Vegetable, and Mineral kingdoms. According to M. Fray, consequently, mineral bodies are organised as well as plants and ani-

\* J. Fred. Blumenbach, *Institutiones Physiologiae*. Goettingae, 1786. Editio quarta. Goettingae, 1821.

† D. J. Frid. Blumenbachii, *Commentatio de Vi Vitali Sanguinis*. Goettingae, 1788. 4to.

‡ *Essai sur l'Origine des Corps Organisés et Inorganisés, et sur quelques Phénomènes de Physiologie Animale et Végétale*. Par J. B. Fray, Commissaire Ordonnateur des Guerres, &c. Paris, 1837. 8vo.

mala. This idea is sufficient to show the capacity of this author for forming notions on the nature of living bodies and the vital principle. The book is altogether absurd, and would deserve no attention, were it not that it is mentioned in respectful terms by Cabanis and Dr Barclay; and that the doctrine of the organisation of stones, rocks, and minerals had been brought forward since that time.

According to Cabanis, it is by movement, progressive and voluntary, that man is enabled to distinguish his own life and that of other animals. Motion is the true sign of the presence of vitality.\* In the most perfect animals, motion and life are communicated to all parts of the body by the nerves, or rather by the nervous system, though life may exist in parts void of nerves. The nervous system is, according to this author, the main principle or agent of life, at least in animals. The sensations are endowed with an individual life; and, in order to live, man, and probably animals, must feel. To live is nothing else but to receive impressions, and to execute the motions which these impressions excite.†

Contemporary with Cabanis were Dumas and Bichat in France; Soemmering, Prochaska, and Reil in Germany; John Hunter and Darwin in England; and it may be added, Rush in the United States. The peculiar views of each of these authors it is impossible here to detail. It is sufficient to mention the following points.

Dumas, who had at the age of eighteen undertaken to examine this question in 1785,‡ gives his matured ideas in 1800. He delineates a correct and instructive picture of vital phenomena, and represents life to consist in four circumstances;—first, the property of resisting decomposition; secondly, the power of preserving the fluidity of the animal liquids, the solidity of the textures and organs, and their mutual correspondence; thirdly, the faculty of taking cognizance of external objects, and being able to approach or avoid them; and, fourthly, the power of maintaining, with other living beings, relations more or less intimate and more or less extensive of a different kind, physiological and moral.§

Bichat died while Cabanis was printing the second edition of

\* *Rapports du Physique et Du Moral de l'Homme*. Par P. J. G. Cabanis, Membre du Senat, &c. Paris, 1802. Second edition. 1805. T. i. p. 93, 245.

† *Ibid*. Tome ii., 137, 287.

‡ *Essai sur la Vie, ou Analyse Raisonnee des Facultes Vitales, pour servir d'explication aux theses soutenues sur le meme sujet dans l'U. de Montpellier*. 14 Janvier 1785.

§ *Principes de Physiologie ou Introduction a la Science Experimentale, Philosophique et Medicale de l'Homme vivant*; par Charles Louis Dumas, de l'Institut National de France Prof. d'Anatomie et de Physiologie, &c. A Paris, 1800. Quatres Tomes. Chapitre iv. et v.

his work. His speculations are too well known to require being mentioned here.

Prochaska placed the principle of life, or *sensorium commune*, in the brain and spinal marrow conjointly. He makes many new and valuable observations on the functions of the nervous system and the presiding power, and is the first who refers the automatic or instinctive motions of Whytt and others to a principle resident in the spinal chord, and which he supposed to act by reflection, or impulse reflected from the surface to that part of the nervous system.\*

The doctrines of Reil upon Irritability and common Sensation (*Koinæsthesis*), were made known by Gautier† of Breslau, and Hubner,‡ his pupils.

Benjamin Rush of Philadelphia published in 1799 some observations, in which he maintained the doctrine of vital principle, as taught by Cullen, and modified by John Brown and his adherents. Rush maintained an opinion, originally taught by Cullen, that life is the effect of certain stimuli acting on the sensibility and excitability, which are extended in different degrees over every external and internal part of the body. These stimuli are as necessary to the existence of life as air is to flame. The stimuli are external and internal. The external stimuli are light, air, heat, sound, colours, exercise, and all agents acting on the senses. The internal stimuli are food, drink, chyle, blood, the various secreted fluids, the moral feelings, and the exercise of the mental faculties. He denies the existence of a principle necessarily and constantly resident in the animal body. It is not self-existent, but depends on the state of the corporeal organs and the operation of stimuli. To admit in the human body an independent principle of life is impossible. All depends on the constant operation of Divine power and goodness.

Part of this doctrine is the same as that afterwards maintained by Treviranus.§

The experimental inquiry of Bichat|| on the comparative necessity of the different organs and functions to life was followed

\* Georgii Prochaska, M. D., Professoris Anatomiae Physiologiae, &c. *Operum Minorum*, Pars ii. Viennae, 1800. *Commentatio de Functionibus Systematis Nervosi*.

† *Dissertatio Inauguralis Med. de Irritabilitatis Notione, Natura et Morbis*. Quam ad honores Doct. Med. et Chirurgiae capessendos, &c. 28vo Augusti, 1793. *Eruditorum Examini subicit Joannes, Ludovicus Gautier, Wratslaviensis. Halae.*

‡ *Cænesthesia Dissertat. Inaug. Quam Praeside, J. Ch. Reil, M. et Ch. D. pro gradu Doctoris, die 23tio Aprilis 1794, Defendit Christianus Fredericus, Hubner. Meso Marchicus. Halae, 1794. 8vo.*

§ *An Inquiry into the Cause of Animal Life. Philadelphia, 1799, and Works*, vol. ii., 2d edit. Philadelphia, 1805.

|| *Recherches Physiologiques sur la Vie et la Mort par Xavier Bichat, Medecin de L'Hotel Dieu, Professeur d' Anatomie de Physiologie et de Medecine. 2d edit. 1802. Troisieme edition. Paris, 1805.*

by similar researches by Nysten\* and Le Gallois;† while Cuvier, by his correct researches in the extensive field of comparative anatomy, and by his philosophical views generally, contributed greatly to rectify many erroneous opinions on the nature of life and the exact influence of the different parts of the organism. Those authors mostly agree in maintaining the dependence of life on organization. But the general result of their observations and reasonings is, to show that the vital principle resides not in any particular organ, but is associated with the individual vital properties and integrity of several organs. Le Gallois maintained that the principle of life is seated in the spinal chord; that the first cause and main agent of the motions of respiration are seated near that part of the *medulla oblongata* which gives origin to the pneumogastric nerve; and that the principle of motion of the sanguiferous system is derived from the whole of the spinal marrow, and not from any particular part of it.

This doctrine has been much canvassed by English physiologists, especially Sir Benjamin Brodie and Dr Wilson Philip. The inferences which the last author has felt himself warranted in establishing are numerous; and some bear not directly on the present subject. Those which relate to it, if modified by the results of the experiments performed by Sir Benjamin Brodie, may be stated in the following manner.

The blood-vessels possess a power capable of supporting a certain degree and form of motion of the blood independently of the heart. The power, both of the heart and blood-vessels, is independent of the brain and spinal chord. The motion of respiration is very requisite to the movements of the circulating organs; and nothing seems so effectually to impair and destroy the action of the movements of the circulating organs as the previous suspension and interruption to the movement of blood through the lungs, and perfect aeration. The movements of respiration depend very much, if not altogether, upon the integrity of certain parts of the brain and spinal chord. The activity of secretion is also much modified by the state of the nervous system.‡

Kurt Sprengel, professor at Halle, is deserving of mention in this place for having given, in 1810 and 1816, a good summary of the views of physiologists on the nature of the vital force, the phenomena by which its presence is manifested, and the laws by which they are regulated. Though entertaining rather confused

\* *Recherches de Physiologie et de Chimie Pathologiques, pour faire suite a celles de Bichat, sur la Vie et la Mort*; par P. H. Nysten, D. M., Professeur de Matière Medicale, &c. A Paris, 1811. 8vo.

† *Expériences sur le Principe de la Vie, notamment sur celui des Movemens du Cœur et sur le siege de ce Principe*. Par M. Le Gallois. Paris, 1812.

‡ *An Experimental Inquiry into the Laws of the Vital Functions*. By A. R. W. Philip, M. D., F. R. S. E., in part republished from *Philosophical Transactions* for 1815, 1817, 1818, &c. Third Edition. London, 1826.



ideas on the nature of organization and on the exact character of organized bodies, he distinguishes between bodies possessed of life and bodies void of life. Though life is a general principle, yet a peculiar life is resident in each organ;—a life in the brain, in the heart, the liver, and in the lungs; and the life of one set of organs is superior to and more energetic than that of another set. The life of the organs now mentioned is superior to that of the cellular tissue.

The vital force is shown by certain effects, and these effects are regulated by certain laws. A certain form of mixture and composition of parts is necessarily united with life. This species of mixture of the liquids and solids it is necessary to preserve in a definite state; and, above all, it is requisite that the vital power be preserved. Any change of the fluids in the way of decomposition indicates a failure of the vital force. Effects of the vital power are animal heat, the constant change of animal matter by absorption and by excretion, and vital turgescence (*turgor vitalis*). The manifestations of life are three,—reproduction, irritability, and sensibility. That the vital power is one and indivisible throughout the entire frame, has been maintained by many, especially the adherents of John Brown. But this, in the sense assigned, seems untenable. At all events, if it be maintained that the vital force is one, the same, and indivisible in the human body, yet it must be allowed that the form of life varies in different organs.

This sort of difference is shown in the receptiveness of certain organs, or their capacity for receiving impressions from stimuli, and in the different degrees of Energy evinced in different organs.\*

It is an objection common to this exposition by Sprengel, and to that by many other authors, that effects and phenomena which are observed in living bodies, they ascribe to a hypothetical principle, which is assumed, probably on inadequate evidence, to exist and operate. To this, however, we shall have occasion soon to direct attention.

Treviranus belongs partly to the beginning of the present century, partly to the close of its first quarter. The first volume of his *Biologie* was published in 1802, and the subsequent volumes during the years between 1802 and 1822,† and his *Phænomena and Laws of Organic Life* in 1832.‡ Life he regards as a state

\* Curtii Sprengel, *Institutiones Medicæ, Physiologia, Volumen Primum*. Paris, 1810. Mediolani, 1816. Cap. ii de Vita Generatim et cap. vi.

† *Biologie, Oder Philosophie der Lebenden Natur für Natur forscher und Aerzte*. Von Gottfried Reinhold Treviranus. Erster Band. Göttingen, 1802. Sechste Bande, 1822.

‡ *Die Erscheinungen und Gesetze des Organischen Lebens*. Neu Dargestellt. Von Gottfried Reinhold Treviranus. Zwey Bande. Bremen, 1832.

of energy or activity, depending not on external agents, but internal, that is, inherent in the living body. The sea, agitated by a tempest, is in a state of energy; but we cannot ascribe to it life, because the agent by which it is so agitated is external, and different from the object itself. At the same time, all the manifestations of the energy of living bodies are results of mutual action between them and the external world, and these are also all mechanical actions.

After showing the insufficiency of the definitions of life given by Kant, Schmid, Erhard, Humboldt, and Schelver, he infers that the only mode of forming and conveying a proper idea of it is by noting the prominent characters of the condition called life. He admits the existence of living or vital power, and he thinks that as to life, organization, and living power, the three following propositions may be established:—

1. Vital power is only there where is matter capable of life. This matter capable of life is a product of the powers of lifeless nature; as soon as it is formed, it is associated with vital force, and this connection raises the vital power from its state of torpor and inaction.

2. Matter susceptible of life is only there where is living power. The former is a product of the latter, and no energy of lifeless nature is capable of bringing forth matter capable of life.

3. Matter capable of life and living power are interchangeable with each other. From the commencement of the general organism, an indissoluble band entwines both. Vital power is at no time separate from matter susceptible of life, neither is the latter ever separate from the former.\*

He further declares life to consist in the conformity of reactions, with dissimilar operations of the material world. The agent or principle which gives to this absolute difference in the strength of external operations relative uniformity is what he names vital or living force, (*Lebens Kraft*).†

In 1822, Dr John Barclay, a popular teacher of anatomy in this city, published an Inquiry into the Opinions, ancient and modern, concerning Life and Organization. The author was a man of great and varied learning, and has produced a very instructive representation of the opinions of various eminent physiological speculators on the nature of organization and the principle of life. He does not himself state distinctly what are his own views. But it is manifest that he is in favour of the idea of vital properties as distinct from physical and chemical properties. In some instances he speaks rather strongly against the speculations of those, whose opinions are at variance with his own. In other instances his keen perception of inconsistency leads him to expose

\* Biologie Erster Band Zweytes Capitel.

† Biologie Drittes Capitel.

with force, and not always without success, the contradictions and errors into which physiologists have been betrayed. The authors whose opinions he has examined, for he has not examined all, he distinguishes into two orders;—those who ascribe to organization the principal phenomena of life; and those who recognize the existence of a living internal principle distinct from the body, and likewise the cause of organization.\*

In his recension of the various authorities under these two heads, Dr Barclay mentions neither Stahl, Hermann Boerhaave, nor Kaau Boerhaave; neither Quesnay, Robert Whytt, Battie, nor Van Geuns; neither Prochaska, Reil, Dumas, Bichat, Rush, Sprengel, Le Gallois, Brodie, nor Wilson Philip; omissions for which, in such a work, it is not easy to account. The writings of these authors form so important a part of the history of physiological opinions, that the omission of them renders imperfect any treatise professing to give a view of the progress of opinion on this subject. This work is, nevertheless, one of great utility to the physiological reader, and would have been of considerable use to Mr Coleridge.

In 1829 appeared a Review of the Doctrine of a Vital Principle, by the late Dr Prichard. In this essay the author considers first the theory of the vital principle, more especially as delivered by John Hunter and his followers; he then examines the proofs and arguments on which the hypothesis is supposed to rest; he examines the comparative force of those facts and arguments which are believed to prove that the spirit or the soul cannot exist separate from matter, and is therefore to be held as the result of organization, and of those which are believed to prove the opposite opinion, that the spirit or soul is immaterial,—leaving readers to draw their own inferences; and he considers how far the hypothesis of a vital principle explains the functions and accounts for the various phenomena of the animal body.

The conclusions which Dr Prichard deduces are the following. The hypothesis of a vital principle is destitute of direct proof; and all that its advocates can adduce in its favour is analogy, that is, the analogy of such principles as are assumed to exist, in order to explain the phenomena of electric and galvanic action. The analogical proof derived from comparison with the spirit or immaterial soul is not less unfounded. Plants and the lower order of animals, as ZOOPHYTA, are void of sensation. The phenomena in animal bodies which have been ascribed to the operation of a vital principle, as those of nutrition, the resistance of chemical action as shown in the integrity of the gastric coat under the solvent power of the gastric juice, and even the plastic or formative power admitted by Blumenbach, must depend on the operation of

\* An Inquiry into the Opinions, Ancient and Modern, concerning Life and Organisation. By John Barclay. M. D., &c. Edinburgh, 1822.

properties and agencies which belong to the highest power and the highest intelligence. This is indeed nothing more or less than the agency of the Deity operating continually through the universe, in preserving and renewing the various tribes of beings. The supposition of a vital principle is, in short, wanting in every characteristic of a legitimate theory. It is not only assumed without adequate proof; but if admitted to exist, it is quite incompetent to explain the phenomena, for the explanation of which its assistance has been invoked.\*

Little doubt can be entertained, that, reasoning strictly and logically, the inference is correct, that the hypothesis of the vital principle is a mere assumption. The question is, in short, can the phenomena and actions taking place in living bodies be explained by means of this principle, or can they be explained without having recourse to it? The former question Dr Prichard answers in the negative; and, with regard to the latter, he shows that no facility is afforded, in explaining the nature and origin of these phenomena, by having recourse to the hypothesis. The vital principle of physiologists does not stand in the same position as the principle of gravitation among physical inquirers, or as the theory of union in definite proportions among chemists. It does not rest on the same solid foundation, does not stand the test of the same trials, neither can it be applied in promoting the advancement of physiological inquiry in aiding the progress of physical and chemical science in the same manner as these principles do. The irregularities in the movements of the planetary system, which were at one time believed to indicate the gradual and sure approach of its parts to destruction, were proved by La Place to be periodical, and to depend on the action of gravitation, itself, modified by that of the centrifugal force. At a period still later, Adams and Leverrier, by the careful study of these irregularities in one planet, showed that they indicated the existence of another planetary body moving in an orbit still more remote, and which, in no long time, was actually discovered. A more decided proof of the existence of a peculiar agent it is scarcely possible to imagine.

Nothing of this kind has ever been known or shown to be true as to the hypothesis of a vital principle.

Physiological authorities nevertheless are not wanting, who think that there are reasons for believing in the existence of this principle, and who accordingly speak of its effects and operation as an established point in science. The sentiments of one of the most recent and rational of these defenders of this doctrine it may be well to adduce, as preliminary to what we have to say on the work of Mr Coleridge:—

\* A Review of the Doctrine of a Vital Principle as maintained by some writers on Physiology, with Observations on the Causes of Physical and Animal Life. By James Cowles Prichard, M. D., F. R. S., &c. London, 1829.

“The word Life, as commonly used, does not denote an individual fact or appearance, and, in the outset of the inquiry, can not be *defined*. It is applied to a certain assemblage and succession of phenomena, which are seen in a great variety of the objects that surround us, and distinguish them from the other objects of our senses. When these phenomena are examined throughout the whole of Nature, it is found that the most general and characteristic of them is, the continued *appropriation* and *assimilation* of surrounding matter, which we call *Nutrition*; a process which maintains a certain definite structure called *Organization*,—which originates, in all cases that can be satisfactorily observed, by *Generation*, *i. e.* by derivation from a previously-existing similar structure,—and terminates by *Death*, *i. e.* not only by cessation of the phenomena in question, but by gradual destruction of the structure exhibiting them, and resolution of the elements composing it into other combinations, presenting no such phenomena.

“Having given this general *description* of what are called Living Bodies, we next observe, that many of the phenomena exhibited by these bodies have been found to be not only inexplicable by, but manifestly inconsistent with, the mechanical and chemical laws that regulate the changes, and have been inferred from the observation of other departments of Nature. In so far as we can ascertain this to be the case, we say that these phenomena are effects of the *Vital Principle*, or of *Vitality*; and that is our *definition* of these terms. They are the general expression for those changes occurring in living bodies, which we judge to be peculiar to them; and stand in the same relation to the science of Physiology, as the terms Chemical Affinity, Electricity, Heat, Light, to other departments of Physical Science.

“Thus defined, the notion of *Vitality* is not only admissible in Physiology, but is that which entitles it to the name of a separate science. Those physiologists, accordingly, who object to the substantive term, *Vitality*, or *Principle of Life*, are obliged to use the adjective *Vital*, which conveys the very same idea. And all that has been stated by Magendie and others, as to the dominion of merely physical laws over living bodies, tends only to limit and define the departments of the animal economy, in which the strictly vital phenomena are observed, not to invalidate what has been said as to their reality and importance.

“This notion of *Vitality*, extending to all classes of organized beings, has no connection whatever with the notion of *Mind*, as distinguished from *Matter*. The phenomena from which the latter notion is deduced, are the characteristic mark of the *Animal Creation* only; and require the admission into the Physiology of Animals, of a class of facts, and a kind of evidence, that have no place in any other physical sciences. Neither does any opinion, or conjecture, that can be formed concerning the essen-

tial nature of Vitality, affect the conclusions in Natural Theology, which are drawn from physiological facts; because these conclusions do not rest on the mode in which Vitality is thought to be communicated to living beings, but simply on the observed adaptation of means to ends, in the economy of living beings.”\*

The foregoing sketch is sufficient to show how many eminent and ingenious men have attempted to define life, and to explain the nature of the vital principle. It shows further, either that Dr Prichard is right in denying the existence of such a principle, or that the whole inquiry is one above and beyond the reach of the human faculties. It has been seen that Blumenbach and Dr Alison admit the impossibility of defining life; and though several definitions have been attempted by various writers, philosophical and physiological, it appears that none is entirely free from objection, and that all attempts to define the nature of life have been unsatisfactory. This achievement, nevertheless, the late Mr Coleridge, not in any way dismayed by the bad success of his predecessors, either philosophical or physiological, has undertaken to perform; and it will not be matter of astonishment to any one who knows its difficulty, that this writer, great as were his talents, has not been more fortunate than others.

The term Life is employed in two senses. In one it is used to denote an unseen principle, which is supposed to animate living bodies, and to which are ascribed the phenomena and actions of living bodies. In the other sense, it is employed to denote a certain state or condition, enduring for a definite period, liable, however, to be shortened or lengthened by various circumstances, and during which take place those phenomena and actions called living.

It is not very easy to understand Mr Coleridge,—a circumstance which we would more willingly ascribe to our own obtuseness, than to any want of clearness or precision in the thinking and mode of expressing his thoughts, employed by that able man. But we may be permitted to express the opinion, that, if he had, in the course of his studies, aimed more at accurate distinction than extensiveness, more at observing and understanding the differences of things than their similitudes, and more at concentration than discursiveness both in thinking and speaking, he would have either formed more correct and practically useful ideas on those objects to which his mind was directed, or he would have seen where definition was practicable, and where it ceased to be so.

Mr Coleridge tells his reader, that he has not the smallest doubt, that the true idea of life existed in the mind of John Hunter; but he thinks it may be doubted, whether the incessant labour in which he was engaged,—whether his constant attention

\* *Outlines of Human Physiology.* By William Pulteney Alison, M. D., F. R. S. E. Third Edition. Edinburgh and London, 1839. See also chapter ii. on the Most General Laws of Vital Action.

to, and occupation with, minute details in living bodies, and bodies that had been living,—permitted him fully to unfold and arrange this idea in distinct, clear, and communicable conceptions. He infers, therefore, that, after all that Hunter has done, it still required some other mind of kindred spirit and similar intellect, but whose genius was not fettered and trammelled by these mechanical details, which Mr Coleridge thinks restrain the movements of genius, to educe from the labours of that anatomist those results which physiological science requires. This, he thinks, was approached and nearly done by Mr Abernethy; but it has never yet been accomplished.

This, the scientific world has, we are informed, yet to see effected; and the author hints that the conviction of this deficient state of physiological philosophy is the main cause of his attempting to enlighten the minds of physiologists on this obscure and not very comprehensible subject.

The definitions given by Bichat, Richerand, and various other modern physiologists, Mr Coleridge rejects, as either mistaking the object, or confounding definition of the vital principle with living bodies, or as being a paraphrase of the term Life. Of other definitions he disapproves, as merely taking some particular function common to living bodies,—for instance, nutrition,—or as substituting an effect, often one single effect, for the cause, or as confounding the law of life, or the primary and universal form of vital agency with the conception of animals, that is, as we infer, living creatures.

After some unnecessary criticism on the tendency which physiologists at the end of the eighteenth and the beginning of the nineteenth century, evinced to seek in chemical action and chemical phenomena, the explanation of all vital processes, and the supposed materialistic tendency of this spirit, he condemns at once the zoo-dynamic theory of Stahl on the one hand, as ascribing vital phenomena to the overruling powers of the rational soul; and, on the other, the undisguised materialism of that hypothesis which teaches that life is the result of organization.

The mode of investigation, and the manner in which the author thinks the physiologist ought to proceed in this inquiry, may be understood from the following extract:—

“The object I have proposed to myself, and wherein its distinction exists, may be thus illustrated. A complex machine is presented to the common view, the moving power of which is hidden. Of those who are studying and examining it, *one* man fixes his attention on some one application of that power on certain effects produced by that particular application, and, on a certain part of the structure, evidently appropriated to the production of these effects, neither the one or other of which he had discovered in a

neighbouring machine, which he at the same time asserts to be quite distinct from the former, and to be moved by a power altogether different, though many of the works and operations are, he admits, common to both machines. In this supposed peculiarity he places the essential character of the former machine, and defines it by the presence of that which is, or which he supposes to be, absent in the latter. Supposing that a stranger to both were about to visit the two machines, this peculiarity would be so far useful, as that it might enable him to distinguish the one from the other, and thus to look in the proper place for whatever else he had heard remarkable concerning either; not that he or his informant would understand the machine any better or otherwise than the common character of a whole class in the nomenclature of botany would enable a person to understand all or any one of the plants contained in that class. But if, on the other hand, the machine in question were such as no man was a stranger to, if even the supposed peculiarity, either by its effects, or by the construction of that portion of the works which produced them, were equally well known to all men, in this case we can conceive no use at all of such a definition; for, at the best, it could only be admitted as a definition for the purposes of nomenclature, which never adds to knowledge, although it may often facilitate its communication. But, in this instance, it would be nomenclature misplaced, and without an object. Such appears to me to be the case with all those definitions which place the essence of life in nutrition, contractility, &c.

“As the second instance, I will take the inventor and maker of the machine himself, who knows its moving power, or perhaps himself constitutes it, who is, as it were, the soul of the work, and in whose mind all its parts, with all their bearings and relations, had pre-existed long before the machine itself had been put together. In him, therefore, there would reside, what it would be presumption to attempt to acquire, or to pretend to communicate, the most perfect insight, not only of the machine itself, and of all its various operations, but of its ultimate principle and its essential causes. The mysterious ground, the efficient causes of vitality, and whether different lives differ absolutely or only in degree, He alone can know who not only said, ‘Let the earth bring forth the living creature, the beast of the earth after his kind, and it was so;’ but who said, ‘Let us make man in our image, who Himself’ breathed into his nostrils the breath of life, and *man* became a living soul.’

“The third case, which I would apply to my own attempt, would be that of the inquirer, who, presuming to know nothing of the power that moves the whole machine, takes those parts of it which are presented to his view, seeks to reduce its various movements to as few and simple laws of motion as possible, and, out of their separate and conjoint action, proceeds to explain and appropriate the structure and relative positions of the works. In obedience to the canon,—‘*Principia non esse multiplicanda præter summam necessitatem, cui suffragamur, non ideo quia causalem in mundo unitatem vel ratione vel experientiâ perspicimus, sed illam ipsam indagamus*



impulsu intellectûs, qui tantundem sibi in explicatione phænomenorum profecisse videtur, quantum ab eodem principio ad plurima rationata descendere ipsi concessum est."—Pp. 37–38.

Mr Coleridge then proceeds in the following manner:—

"What is life? Were such a question proposed, we should be tempted to answer, what is *not* life that really *is*? Our reason convinces us that the quantities of things, taken abstractedly as quantity, exist only in the relations they bear to the percipient; in plainer words, they exist only in our minds, *ut quorum esse est percipi*. For if the definite quantities have a ground, and, therefore, a reality, in the external world, and independent of the mind that perceives them, this ground is *ipso facto* a quality; the very etymon of this world showing that a quality, not taken in its own nature, but in relation to another thing, is to be defined *causa sufficiens, entia, de quibus loquimur; esse talia, qualia sunt*. Either the quantities perceived exist only in the perception, or they have likewise a real existence. In the former case, the quality (the word is here used in an active sense) that determines them belongs to life, *per ipsam hypothesin*; and, in the other case, since by the agreement of all parties, life may exist in other forms than those of consciousness, or even of sensibility, the *onus probandi* falls on those who assert of any quality that it is *not* life. For the analogy of all that we know is clearly in favour of the contrary supposition; and if a man would analyse the meaning of his own words, and carefully distinguish his perceptions and sensations from the external cause exciting them, and, at the same time, from the quantity or superficialities under which that cause is acting, he would instantly find himself, if we mistake not, involuntarily identifying the ideas of quality and life. Life, it is admitted on all hands, does not necessarily imply consciousness or sensibility; and we, for our parts, cannot see that the irritability which metals manifest to galvanism can be more remote from that which may be supposed to exist in the tribe of lichens, or in the *helvellæ, pezizæ, &c.*, than the latter is from the phenomena of excitability in the human body, whatever name it may be called by, or in whatever way it may modify itself. That the mere act of growth does not constitute the idea of life, or the absence of that act exclude it; we have a proof in every egg before it is placed under the hen, and in every grain of corn before it is put into the soil. All that could be deduced by fair reasoning would amount to this only, that the life of metals, as the power which effects and determines their comparative cohesion, ductility, &c., was yet lower on the scale than the life which produces the first attempts of organization, in the almost shapeless tremella, or in such fungi as grow in the dark recesses of the mine."—Pp. 38–40.

From the foregoing observations it results that Mr Coleridge, notwithstanding the philosophical air with which he rejects the definitions of other physiological authorities, gives views of the nature of life and living bodies not less erroneous than they have

done. It appears, indeed, that he ascribes life not only to plants and animals, but to minerals and metals, to rocks and stones,—in short, to objects which may doubtless be said to exist, but which cannot, according to the just and correct acceptance of the term, be said to live. It is evident, therefore, that Mr Coleridge confounds the idea of life with that of existence, and ascribes to all things which have substance and material form, the property of life. This, we must be permitted to say, is a most illogical and erroneous view, and one which has scarcely the advantage of plausibility to recommend it.

To us it appears not doubtful, that Mr Coleridge, in this mode of contemplating life and living objects, confounds life, as we have said, with existence. All objects in this world,—rocks, stones, minerals, metals, the atmosphere itself, all gaseous bodies,—may be said to exist; that is, they possess a true material existence, and they have certain properties which are called material, physical, or chemical, according to their effects. They have weight and impenetrability. Some sink in water and some float in the atmosphere. Some are undergoing constant chemical changes. But none of these circumstances prove them to be the seat of those processes, in which if life does not consist, at least they denote the presence of vital action. A crystal of quartz or amethyst, with its geometrical planes and angles, is certainly a very symmetrical, and therefore an interesting and beautiful object, and calculated to make a deep and strong impression on the mind, as if it were an effect and proof of vital action, and the presence of life. But, so far as is known, such crystals are not effects of vital action, and never have been effects of vital action; nor can the objects in which they appear be regarded, as Mr Coleridge regards them, as living. These bodies are regulated by totally different laws; and though it may be allowed that they are the creation of a living Being, it can never be admitted, without the greatest confusion and disorder, that they are or have been living at any time themselves.

This doctrine is, nevertheless, not at all new. Traces of it, we believe, may be found in various ancient physiologists; in not a few of those who have written since the revival of literature; and in one writer, not very remote from the present time, the doctrine, it has been shown, was explicitly taught.\* But to these authorities it is unnecessary to have recourse for the proof of what we state. We shall confine this part of the question to a period more recent, and to authority which, however we may differ from it, is certainly entitled to attention.

Exactly forty-nine years ago, namely, the last year of the eighteenth century, appeared a work, entitled, “Dissertations on Inflammation, by John Burns,” at that time a surgeon in Glas-

\* See page 410.

gow, and at present the venerable and highly-respected professor of surgery in the University of that city. In the preliminary dissertation of that treatise, which is certainly a work of great merit, the doctrine of Basaltic Rocks and Crystalline Bodies in general being organized and possessed of life, or under the influence of life, is most distinctly delivered and illustrated. After stating that natural historians divide the objects of their examinations into three great classes, called the kingdoms of nature, namely, the Animal, the Vegetable, and the Mineral, Dr Burns adds;—

“ Amongst all the different individuals of these divisions, an organized form regularly prevails. A stone or a salt assumes uniformly an appearance which continues always the same in similar species. The vast rocks of Basaltes, which stand as so many monuments of the dreadful cause which produced them, consist of large pillars, having five sides of unequal dimensions. Siliceous crystals exhibit the form of hexagonal pyramids, whilst the zeolite assumes the figure of a beautiful star, and the amianthus that of regular and parallel fibres.

“ The vegetable has likewise its appropriate construction, which, as in the other kingdoms, continues always the same ; but in this respect it differs from the mineral, that to a regular figure there are conjoined organic vessels to maintain and increase that form, according to the uniform action of a certain principle which they possess, and which has been called life.”\*

Of the third class, or Mineral Kingdom, he afterwards gives the following statement. “ The individuals of the last class have promiscuously received the name of dead matter, because, when compared with the other classes, they appeared to be inanimate. But, by a more accurate examination, we shall find, that there is in reality no individual whatever in any of the kingdoms or classes of nature which can be called inert, or truly dead. For, from the largest masses of matter down to the most minute corpuscles, we distinctly perceive the operation of an active and immaterial principle.

“ This principle has received different names, according as it manifests itself :—The power which supports the functions (if I may use the expression), and regulates the motions and actions of the different parts of the universe, has been called gravitation ; whilst the property which makes one particle unite with another, and which modifies the form, and produces the increase of the smaller bodies, has received the name of corpuscular attraction. The principle, again, which regulates the intestine changes, and determines the combinations and specific states of existence in matter, has been denominated elective or chemical attraction.

“ How matter, which, by itself, and viewed abstractedly, must be considered as inert and dead, should be capable of combining

\* Dissertations on Inflammation, Vol. i. By John Burns, Surgeon in Glasgow. Glasgow, 1800. 8vo. Dissert. i. p. 2 and 3.

with this active and immaterial power, it is impossible to say. But having from the creation perceived this union, we find it to be now impracticable to form a conception of matter unconnected with this property.

“ Without this enlivening principle, all nature must be dead ; and matter deprived of it must either cease to exist, or exist in a way which we cannot possibly comprehend. The union of the whole universe must be dissolved, and the beautiful dependence of one part on the rest for ever destroyed. We know, and are taught to believe, that the Great Being who formerly sent forth this active vital power, and bade the worlds live, will one day recal His gift. Matter shall then cease altogether to be, or shall return to that unknown chaotic state which poets have imagined, and vainly attempted to describe. The particular laws and operations of attraction, or the life of matter, belong for investigation to the natural philosopher ; and therefore it will here only be proper to remark, that I consider animals, vegetables, and what is called inanimate matter, as all possessing an immaterial principle, differing greatly indeed in its nature and effects in these different classes, but still deserving, in all of them, the name of life, being of equal value to each, and absolutely requisite for their preservation.”\*

This power, Dr Burns allows, is exceedingly simple in common matter, that is, in what has generally been called inanimate matter. But he maintains, that it differs not in kind but in degree, in the vegetable and animal world.

Here, then, is the idea of life being distributed in, and possessed by, granite mountains, strata of gneiss, and mica slate, and, in short, the whole mineral world distinctly stated and applied. If on this point any doubt were left, it would be removed by the following passages:—

“ We have, then, in the three classes of nature, three different species of life, each more perfect than the other, and increasing gradually in their delicacy and intricacy. The most simple, or that of matter, is the most permanent, and on it the other two are built. I do not, however, mean from this to say, that attraction, or the vital principle of matter, acts in the higher classes merely as attraction ; or, in other words, that it remains unchanged in its properties. On the contrary, a very great alteration takes place ; and, although it still possesses the quality expressed by the word attraction, making the parts adhere together, it yet is so far changed and perfected as to exhibit many other phenomena and new actions, of which, before this elevation, it was incapable. It is rendered not only more perfect, in this respect, of gaining new properties, but even its original quality of producing attraction is much improved ; for a living muscle will bear a greater weight, without laceration, than one newly dead, or one called, in common

\* *Dissertations on Inflammation, &c.* By J. Burns. Dissert i. p. 6, 7, 8.

language, dead ; which proves, that the vital principle in animals has a greater power of producing the effect called attraction than the vital principle of common matter.

“ The three classes run imperceptibly into each other ; and thus we have a complete chain of existence established, from the most simple to the most complicated body. Those species of amianthus, which are called mountain cork, although minerals, yet resemble vegetables so strongly, that they link the classes together ; whilst the corallines, although animals, resemble both minerals and vegetables. The fungi, though plants, consist of the same principles with animals. Some of the fuci resemble hair and other cartilage ; whilst the sponge, although an animal, grows like a vegetable. Not only the external and chemical qualities, but also the vital principle and its phenomena, form a regular gradation in the different classes. Even in a single individual of the two higher classes, we perceive the gradual elevation of one species of life into another. This change uniformly begins in the fluid part of the individual. In the plant, the first change produced on common matter, or the aliment, is its conversion into vegetable juices, which exhibit certain vital phenomena different from common fluid matter : afterwards organization is added, in which the vegetable life is exhibited with most perfection. In animals, again, there is a greater gradation, and more complicated change, before animal life be imparted in its greatest perfection. The food is first converted into chyle, which has a lower species of life than the blood, which again is more imperfect in its vitality than the organised parts. The living principle of the blood differs, both in degree and kind, from that which belongs peculiarly to the animal ; but these two are connected, and react on each other.”\*

The following passage, which contains the ideas of the author on the nature of life, may justly be compared with various of the statements given by Mr Coleridge. They have certainly the advantage of being given in language more clear, more definite, and more intelligible :—

“ Life is a principle which we can only detect and judge of by its operations or actions ; and, when these are not exhibited, we are apt, though sometimes erroneously, to conclude, that the body is dead. The leading property of life is to communicate a preservative power to every individual with which it is connected. This is sometimes effected by very evident and intricate actions ; but at other times is exerted without any sensible operation. The crystal resists, to a certain degree, mechanical impressions which would destroy its form. The blood when newly drawn, the sap of vegetables, the living egg, resist cold, by an imperceptible operation, to a much greater degree, when alive, than after their peculiar life departs. They resist all the efforts of chemical agents

\* Dissertations, &c. p. 11, 12, 13.

which act on common matter ; nor is it possible to decompose or injure them in this way until they lose their specific life, and descend in the scale of existence. This simple preservative power is a discriminating mark of the presence of life ; but we cannot detect it until we apply destroying causes. It is the uniform and universal effect of the combination of life with any substance, whatever its nature or structure may be. It is the essential characteristic of life, which it must show whenever it is present. But when we find vital power united with a certain organisation, then more varied phenomena take place ; and these are called actions of the vital principle. In the plant, the bud expands, the stem shoots up, the food is absorbed, digested, and circulated ; air is thrown out, and particular secretions take place. In the animal, these actions are still more evident, but more intricate, and infinitely more varied. They are the support of our health, and the source of all our disease."

By why do we refer to modern writers for proofs of the antiquity of this doctrine ? It may be said to be so ancient, that it was the first idea familiar to the human mind. If we examine the history of nations and languages, and trace the progress of man from his first appearance in the woods as a naked, untaught savage, we shall find that the idea of ascribing life and living actions to all bodies and objects, animate and inanimate alike, is the first that takes possession of his mind, is the most familiar, continues longest, and is that which it requires the most lengthened education and the most sedulous mental cultivation to eradicate. A savage regards the mountain and its rocks, the lake and its waters, and the river, as all equally living. The sun, moon, and stars it is most natural that he should regard as alive, as the first gives him both light and heat, and seems to him the great fostering parent of animals and plants ; the second illuminates his wanderings during the night ; and the stars upon which he often gazes are to him not only objects of beauty, but the means of guiding him in the forest and over the mountain. Not only does the uncivilised man regard these objects as endowed with life, but to them he ascribes supernatural powers, and before them as such bows in worship and adoration. Thus it is, that, in the most ancient languages, the sun, moon, and heavenly bodies are invariably named and often addressed as living beings, as divinities possessing powers capable of influencing the condition of the human mind ; that the river is supposed to be under the dominion of a tutelar god ; and that the mountain, the rock, and the cavern are equally supposed to possess life, and to be capable of being addressed as living beings. The lightning, the wind, the fire, are, in like manner, from the undeniable force which they often display, naturally supposed to be alive ; and are invariably spoken of

as living beings of irresistible power. These conceptions are the natural offspring of the mind untrained, and incapable of distinguishing between bodies living and bodies not endowed with life. The savage is most familiar with the idea of life and action as it appears in his own person and that of his companions, and in the animals most known to him. He knows they are alive. He knows that the time was, at which they were not alive; and he is aware that the time will come, when they cease to live in the present form. This idea of life, therefore, he transfers, and applies to other objects near and remote; and to these he ascribes qualities and actions similar to those which he observes in himself, his fellow-creatures, and the dumb animals with which he associates.

At a similar stage of his history, he fashions various images and figures of the objects of his reverence, his worship, and his expectation; and ascribes to these images the possession of vital powers and vital actions. The rock of marble or block of wood from which he moulds these images, he regards as alive quite as much as himself, and the beings they are supposed to represent.

In a manner not dissimilar, and from the same incapacity of forming correct distinctions, we find children ascribe life and living properties to stones and to articles of furniture which are void of life. Nothing is more common than for a child to regard a stone or a toy as alive; nothing more usual than for savages and children to speak of a watch as a living object; and the story of the savage, who thought the watch died when it ceased to beat is true, not of one savage, but of whole nations.

This mode of thinking, and the error which it implies, continues for ages and generations with savages, who are unable to form those distinctions and abstractions which enable them to understand the difference between living bodies and those not possessed of life. With children it would probably continue equally long, but that they are placed in situations and amidst society, in which this mental process is naturally rendered more easy and more rapid, and in which they are earlier taught to distinguish animate and inanimate objects from each other. Yet even that process is not always, even in the favourable circumstances of civilized society and educated fellow creatures, accomplished with perfect accuracy and complete justice. Many persons who know that animals are endowed with life, think that trees and plants and various vegetable bodies are not endowed with life. This, however, arises from their possessing limited and inaccurate notions of the nature of life and living actions. They suppose that life is to be always indicated by consciousness, by sensation, perception, and motions perceptible to sight. They cannot imagine that what does not move, does not see, hear, or feel, can be alive; and they consequently ascribe a sort of nega-

tive life to plants and trees, while they hesitate not to consider animals as the only beings properly endowed with life in its exquisite and perfect character. We have even known persons who refused to allow fishes to be animals,—we suppose, because they are altogether speechless.

In short, it must be clear to every one who is familiar with inquiries of this kind, that to ascribe life to all matter indiscriminately, to the mineral world as well as to the vegetable and animal, is the character of the human mind in the early and infantile period, as it were, of its growth; that the idea is a mistake or error proceeding from limited and inadequate observation; that it is only rectified, as the mind becomes habituated to compare different objects, as it extends its sphere of observation and increases its amount of knowledge; and that, consequently, to restore this idea after it has been rejected as unfounded, is rather to retrograde than to advance in scientific distinction. Such, nevertheless, is the method adopted by Mr Coleridge in the present treatise. The general result of his reasonings is given in the following passages:—

“ From the preceding, it should appear that the most comprehensive formula to which life is reducible, would be that of the internal copula of bodies, or (if we may venture to borrow a phrase from the Platonic school) the *power* which discloses itself from within as a principle of *unity* in the *many*. But that there is a physiognomy in words, which, without reference to their fitness or necessity, make unfavourable as well as favourable impressions, and that every unusual term, in an abstruse research, incurs the risk of being denominated jargon. I should, at the same time, have borrowed a scholastic *term*, and defined life *absolutely*, as the principle of unity in *multeity*, as far as the former, the unity to wit, is produced *ab intra*; but *eminently* (*sensu eminenti*), I define life as the *principle of individuation*, or the power which unites a given *all* into a *whole* that is presupposed by all its parts. The link that combines the two, and acts throughout both, will, of course, be defined by the *tendency to individuation*. Thus, from its utmost *latency*, in which life is one with the elementary powers of mechanism, that is, with the powers of mechanism considered as qualitative and actually synthetic, to its highest manifestation (in which, as the *vis vitæ vivida*, or life as life, it subordinates and modifies these powers, becoming contra-distinguished from mechanism, *ab extra*, under the form of organization), there is an ascending series of intermediate classes, and of analogous gradations in each class. To a reflecting mind, indeed, the very fact that the powers peculiar to life in living animals *include* cohesion, elasticity, &c. (or, in the words of a late publication, ‘that living matter exhibits these physical properties,’ (would demonstrate, that, in the truth of things,



they are homogeneous, and that both the classes are but degrees and different dignities of one and the same tendency."—Pp. 41–43.

Again,—

"In all cases, that which, *abstractly* taken, is the definition of the *kind*, will, when applied *absolutely*, or in its fullest sense, be the definition of the highest *degree* of that kind. If life, in general, be defined *vis ab intra, cujus proprium est coadunare plura in rem unicam, quantum est res unica* ; the unity will be more intense in proportion as it constitutes each particular thing a whole of itself ; and yet more, again, in proportion to the number and interdependence of the parts, which it unites as a whole. But a whole, composed *ab intra*, of different parts, so far interdependent, that each is reciprocally means and end, is an individual, and the individuality is most intense where the greatest dependence of the parts on the whole is combined with the greatest dependence of the whole on its parts ; the first (namely, the dependence of the parts on the whole) being absolute ; the second (namely, the dependence of the whole on its parts) being proportional to the importance of the relation which the parts have to the whole, that is, as their action extends more or less beyond themselves. For this spirit of the whole is most expressed in that part which derives its importance as an end from its importance as a mean, relatively to all the parts under the same copula."—P. 44.

It is not to be doubted that all this is exceedingly logical, philosophical, and excogitated according to the approved principles of the art of reasoning. It is, nevertheless, so far as it is intelligible, neither new nor useful ; and it certainly fails most completely in the great object which the author had in view, that of communicating a clear, distinct, and instructive idea of what is meant by the abstraction, *Life*. It is, therefore, justice to the author to see if, in the further pursuit of his inquiry, he contributes any elucidations more intelligible. The following, we presume, is offered in this point of view :—

"Exclusively, therefore, for the purposes of *illustration*, I would take, as an instance of the first step, the metals, those, namely, that are capable of permanent reduction. For, by the established laws of nomenclature, the others (as sodium, potassium, calcium, silicium, &c.) would be entitled to a class of their own, under the name of *bases*. It is long since the chemists have despaired of decomposing this class of bodies. They still remain, one and all, as elements or simple bodies, though, on the principles of the corpuscularian philosophy, nothing can be more improbable than that they really are such ; and no reason has or can be assigned on the grounds of that system, why, in no one instance, the contrary has not been proved. But this is at once explained, if we assume them as the simplest form of unity, namely, the unity of powers and properties. For these, it is evident, may be endlessly modified, but can never be decomposed. If I were asked by a philosopher who

had previously extended the attribute of life to the *Byssus speciosa*, and even to the crustaceous matter, or outward bones of a lobster, &c., whether the ingot of gold expressed *life*, I should answer, without hesitation, as to the *ingot* of gold assuredly not, for its form is accidental and *ab extra*. It may be added to or detracted from, without in the least affecting the nature, state, or properties in the specific matter of which the ingot consists. But as *gold*, as that special union of absolute and of relative gravity, ductility, and hardness, which, wherever they are found, constitute *gold*, I should answer, no less fearlessly, in the affirmative. But I should further add, that, of the two counteracting tendencies of nature, namely, that of *detachment* from the universal life, which universality is represented to us by gravitation, and that of *attachment*, or reduction into it; this and the other noble metals represented the units, in which the latter tendency, namely, that of identity with the life of nature, subsisted in the greatest overbalance over the former. It is the form of unity, with the least degree of tendency to individuation.

From the foregoing distinctions it results, that a mass of gold from the Ural mountains is dead; while a sovereign from the mint is alive.

“Rising in the ascent, I should take, as illustrative of the second step, the various forms of crystals as a union, not of powers only, but of parts, and as the simplest forms of composition in the next narrowest sphere of affinity. Here the form, or apparent *quantity*, is manifestly the result of the *quality*, and the chemist himself not seldom admits them as infallible characters of the substances united in the whole of a given crystal.

“In the first step, we had life, as the mere *unity* of powers; in the second, we have the simplest forms of *totality* evolved. The third step is presented to us in those vast formations, the tracing of which generically would form the science of geology, or its history in the strict sense of the word, even as their description and diagnostics constitute its preliminaries.

“Their claim to this rank I cannot here even attempt to support. It will be sufficient to explain my reason for having assigned it to them, by the avowal, that I regard them in a twofold point of view: *1st*, as the residue and product of vegetable and animal life; *2d*, as manifesting the tendencies of the life of nature to vegetation or animalization. And this process I believe,—in one instance, by the peat morasses of the northern, and, in the other instance, by the coral banks of the southern hemisphere,—to be still connected with the present order of vegetable and animal life, which constitute the fourth and last step in these wide and comprehensive divisions.

“In the lowest forms of the vegetable and animal world, we perceive totality dawning into *individuation*, while, in man, as the highest of the class, the individuality is not only perfected in its corporeal sense, but begins a new series beyond the appropriate limits of physiology. The tendency to individuation, more or less

obscure, more or less obvious, constitutes the common character of all classes, as far as they maintain for themselves a distinction from the universal life of the planet ; while the degrees, both of intensity and extension, to which this intensity is realized, form the species and their ranks in the great scale of ascent and expansion."—Pp. 46–48.

The author is aware of the difficulties of the task which he undertakes, and entreats the indulgence of readers.

"In the treatment of a subject so vast and complex, within the limits prescribed for an essay like the present, where it is impossible not to say either too much or too little (and too much because too little), an author is entitled to make large claims on the candour of his judges. Many things he must express inaccurately, not from ignorance or oversight, but because the more precise expression would have involved the necessity of a further explanation, and this another, even to the first elements of the science. This is an inconvenience which presses on the analytic method, on however large a scale it may be conducted, compared with the synthetic ; and it must bear with a tenfold weight in the present instance, where we are not permitted to avail ourselves of its usual advantages as a counterbalance to its inherent defects. I shall have done all that I dared propose to myself, or that can be justly demanded of me by others, if I have succeeded in conveying a sufficiently clear, though indistinct and inadequate notion, so as of its many results to render intelligible that one which I am to apply to my particular subject, not as a truth already demonstrated, but as an hypothesis, which pretends to no higher merit than that of explaining the particular class of phenomena to which it is applied, and asks no other reward than a presumption in favour of the general system of which it affirms itself to be a dependent though integral part. By life I everywhere mean the true idea of life, or that most general form under which life manifests itself to us, which includes all its other forms. This I have stated to be the *tendency to individuation*, and the degrees or intensities of life to consist in the progressive realization of this tendency."—Pp. 48, 49.

The conclusion is afterwards stated summarily in the following manner :—

"Life, then, we consider as the copula, or the unity of thesis and antithesis, position and counterposition,—life itself being the positive of both ; as, on the other hand, the two counterpoints are the necessary conditions of the *manifestations* of life. These, by the same necessity, unite in a synthesis ; which again, by the law of dualism, essential to all actual existence, expands, or *produces* itself, from the point into the *line*, in order again to converge, as the initiation of the same productive process in some intenser form of reality. Thus, in the identity of the two counter-powers, life *subsists* ; in their strife it *consists* : and in their reconciliation it at once dies and is born again into a new form, either falling back into

the life of the whole, or starting anew in the process of individuation."—Pp. 51, 52.

The discussion which follows on space, time, and motion, or magnetic force, polarity, persons with ossified imaginative powers, we pass over, partly as unsuited to our pages, partly as instances of unprofitable speculation. The author next discusses the merit of some of the peculiar doctrines of Mr Lawrence, which certainly cannot at the present time be seasonable. The trains of reasoning, in short, are so peculiar, that it is entirely impossible to trace the connection between the premises and the conclusions. For instance, what can be thought of the following sentences, which we acknowledge to be far beyond our comprehension?

"I can now, for the first time, give to my opinions that degree of intelligibility, which is requisite for their introduction as hypotheses; the experiments above related, understood as in the common mode of thinking, prove that the magnetic influence flows in length, the electric fluid by suffusion, and that chemical agency (whatever the main agent may be) is qualitative and *in intimis*. Now my hypothesis demands the converse of all this. I affirm that a power, acting exclusively in length, is (wherever it be found) *magnetism*; that a power which acts *both* in length and in breadth, and *only* in length and breadth, is (wherever it be found) *electricity*; and finally, that a power which, together with length and breadth, includes depth likewise, is (wherever it be found) *constructive agency*. That is but *one* phenomenon of magnetism, to which we have appropriated and confined the term magnetism; because, of all the natural bodies at present known, iron, and one or two of its nearest relatives in the family of hard yet coherent metals, are the only ones in which all the conditions are collected, under which alone the magnetic agency can appear in and during the act itself. When, therefore, I affirm the power of reproduction in organized bodies to be magnetism, I must be understood to mean that this power, as it exists in the magnet, and which we there (to use a strong phrase) catch in the very act, is to the same kind of power, working as reproductive, what the root is to the cube of that root. We no more confound the force in the compass needle with that of reproduction, than a man can be said to confound his liver with a lichen, because he affirms that both of them grow."—Pp. 91, 92.

If this be philosophy, blessed, it may be said, are they who know it not.

The conclusion of the whole matter is, we understand, intended in the following passage:—

"My hypothesis will, therefore, be thus expressed, that the constituent forces of life in the human living body are,—first, the power of length, or REPRODUCTION; second, the power of surface (that is, length and breadth), or IRRITABILITY; third, the power of depth, or SENSIBILITY. With this observation I may conclude

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these remarks, only reminding the reader that life itself is neither of these separately, but the copula of all three,—that life, *as* life, supposes a positive or universal principle in nature, with a negative principle in every particular animal, the latter or limitative power constantly acting to individualize, and, as it were, *figure* the former. *Thus*, then, life itself is not a *thing*,—a self-subsistent *hypostasis*,—but an *act* and *process*; which, pitiable as the prejudice will appear to the *forts esprits*, is a great deal more than either my reason would authorise or my conscience allow me to assert—concerning the soul, as the principle both of reason and conscience.” —Pp. 93, 94.

Such is the theory of life propounded by that distinguished man and original discourses, Samuel T. Coleridge. Often, in reading it, have we been tempted to ask, is this really the production of that highly-esteemed intellect, or is it the bold effusion of some small wit, who, anxious to see the length to which human credulity can go, ascribes to the genius of the illustrious dead, the wild and irrational extravagances of the obscure living? If this alleged theory of life be, as represented, the production of the mind of Coleridge, it only adds one more to the examples of the inadequacy of the human intellect to excel in all branches of science, or even to attain to moderately correct views in a department to which the mind of its author had not been trained, and to study the difficulties of which require greater leisure than his multifarious and desultory pursuits could permit. To few, indeed, is it given to excel in more than one department of science at once; and the alleged examples of this excellence, and especially of any correct or just views in divisions of science not very closely connected with the pursuits of the individual, have always appeared to us more than doubtful.

Inquiries into the causes of the phenomena of living bodies are so peculiar, and attended with so great difficulty, that it ought not to be matter of astonishment, that those most familiar with them have failed to penetrate their mystery. Much less can it be matter of wonder, that minds of the greatest energy and the most exalted power, when little accustomed to such inquiries, and little acquainted with the multiplied sources of fallacy with which they are beset, should go astray, and only add to the confusion and mystery natural to the subject.

Mr Coleridge was evidently a man ambitious of being thought to have applied philosophy to all branches of human knowledge; and, if the present be really his treatise, it shows that he had imagined that he had mastered one of the most difficult problems in general physiology. The only practical conclusion which is deducible from its perusal is, either that the author has most grievously failed,—or that the subject, as proposed, is entirely incapable of being explained and comprehended by the human fa-

culties. It would be unjust to say, that the performance is all confidence, or that the author never seems to doubt his own capacity for the task which he undertakes. Yet no idea seems to be more familiar to him, than that all other physiologists have failed, and that he must succeed. Nor does it ever seem to occur to him, that the numerous failures by eminent thinkers, might render it probable, that the question is rather difficult for distinct comprehension and solution by the intellect of man. Assuredly there is no department of human knowledge in which caution and humility, and we may add, diffidence, are so becoming, if not indispensable, as in the application of metaphysical reasoning to questions in general physiology.

When the mind attempts to rise from the ordinary details of the structure and functions of Living Bodies to the investigation of the cause of these functions, and the connection between the cause and the structure, it gets bewildered in an endless maze of complicated phenomena, all of which appear to be intimately connected, but which it is impossible to trace to any single cause or moving agent, other than the will of the Creator. While one speculator places life and the cause of vital action in the blood, another fixes it in the nervous system, a third in electricity, a fourth in magnetism or electro-magnetism, and a fifth in chemical attraction. One theorist points to a particular spot in the brain; another represents the spinal chord to be the seat of vital action; a third places his finger on the solar plexus, and here he says is the seat of life. It is, meanwhile, manifest, that it is impossible to detect the subtile essence in any one of these situations, and that, if life or the cause of vital action reside in any one organ or any one individual point, it is not in such a shape that its existence or even operation and effects can be demonstrated; and that, further, there is, as Hunter used to say, a *materia vitæ diffusa*, or a diffuse principle of life, as well as a definite one.

When on this subject, we are involuntarily led to inquire, what course would have been adopted by another profound thinker of the last age, and one who, by education and habits, was well qualified to give a sound opinion? Would Sir James Mackintosh have allowed himself to wander in the labyrinth in which we find the author of the present treatise losing his way? Every cautious philosophical inquirer chooses his own subject, and selects or avoids questions according as he sees they are either capable of solution, or admit of illustration or limitation; and probably that practised thinker would have avoided such a subject, unless it had been thrust upon him. Yet, if we do not greatly misunderstand the writings and mode of thinking of that old man eloquent, sure we are that neither would he have involved the present question in confusion and disorder by ascribing life and living pro-

perties to rocks, to stones, to minerals, and metals, nor would he have allowed his inquiries to involve himself in contradictions and obscurity. It had been well for Mr Coleridge had he been able to imitate the cautious reserve and the precise mode of thinking and reasoning which distinguished the man whose faculties Mr Coleridge affected to depreciate by characterising his head as a warehouse to let. Never was witticism less happily applied. Never was truth so sacrificed to the play of words. If there was one quality in which Sir James Mackintosh excelled above another, it was in that of mental analysis and methodical arrangement, of clearness in conception, and distinctness of representation. The readers of the present treatise have the means of determining whether Mr Coleridge was equally gifted in the power of correct thoughts and just reasoning.

It must, nevertheless, be allowed, that the subject seems little calculated to invite inquiry. That it is curious and perhaps interesting, as a matter of speculation, cannot be denied. That it promises, even if more fully understood, to furnish useful practical results, will hardly be admitted. It seems also a proof of the inutility of the inquiry, that all the most able and best informed physiologists have of late years tacitly abandoned all definition and inquiry into the nature of life, and appear to have given up the question, as one either of no practical value if solved, or as not admitting of solution in the manner in which it is usually stated. Since the appearance of the treatise of Bichat and the elements of Richerand, no physiologist of much eminence has attempted either definition or description; and we infer, that this is proof that the inquiry is unprofitable.

ART. II.—1. *On the Physiology of Cells, with a view to elucidate the Laws regulating the Structure and Function of Glands.*

By THOMAS WILLIAMS, M. D., London. Guy's Hospital Reports, Second Series, Vol. iv. London, 1846. 8vo, pp. 273—331.

2. *On the Nucleus of the Animal and Vegetable Cell.* By MARTIN BARRY, M. D., F. R. S. Edinburgh New Philosophical Journal, October 1847. Edinburgh, 1847. 8vo, pp. 29.

3. *Recent Advances on the Physiology of Motion, the Senses, Generation, and Development.* By WILLIAM BALY, M. D., F. R. S., Physician to Millbank Prison, and Lecturer on Forensic Medicine at St Bartholomew's Hospital; and WILLIAM

SENHOUSE KIRKES, M. D. Being a Supplement to the second Volume of Professor Müller's *Elements of Physiology*. London, 1848. 8vo, pp. 182.

4. *Outlines of Physiology, for the Use of Students*. Parts I. and II. By ALLEN THOMSON, M. D., Professor of Anatomy in the University of Glasgow. 12mo, pp. 308. January and July 1848.
5. *Hand-Book of Physiology*. By WILLIAM SENHOUSE KIRKES, M. D. Assisted by JAMES PAGET, Lecturer on General Anatomy and Physiology at St Bartholomew's Hospital. With Illustrations on steel and wood. London, 1848. 12mo, pp. 705.

FOR a long series of years, the chief treatises employed by English students of physiology were the *Summary of Magendie*, the *Institutions of Blumenbach*, as annotated, illustrated, and expanded by Dr Elliotson, and the useful work of Dr Herbert Mayo. The first, though a work of great originality, has long ceased to be looked on as giving the most recent views of the science; and indeed the author has shown, by publishing no new edition for years, that he considers it as no longer capable of being accommodated to the present time. The last edition of the *Institutions of Blumenbach* by Dr Elliotson is a very excellent treatise,—learned and instructive,—and may be studied at present with great advantage. The *Elements* of Dr Mayo were, in a great degree, supplanted in the London schools by the treatise of Muller, which promised to be capable for a long time of satisfying the wants of physiological readers, and which must always be studied with interest by those who wish information at once extensive and correct.

Since the appearance of the translation of the work of Muller by Dr Baly, there has been no good treatise on physiology which could be recommended to students, except the translation of Wagner's *Elements* by Dr Willis. The moderate limits within which that work is confined, the methodical order in which it is arranged, as well as the substantial information which it contains, recommend it strongly to the student who is undergoing the process of initiation into the elements of physiological knowledge. The elaborate and comprehensive treatise of Muller, we have always said, is a most valuable repository of information. But very much as a repository has it been treated. It has been consulted more than studied; and, while it has been pretty freely borrowed from, it has been little read by students, and not much perused by practitioners. The principal reason of this is its size and extent, and the minuteness of its details, which have, in a great measure, deterred



from the consecutive and systematic perusal of its pages, students indolent, superficial, and unambitious of a high standard of excellence, and practitioners who have little leisure, and whose zeal is insufficient to make them acquire knowledge amidst difficulties.

In addition to this circumstance, it must be allowed, that so rapid has been the progress of physiological knowledge during the last seven or eight years, and so many new views and additional facts have contributed to enrich various departments of the science, that this treatise, comprehensive as it is, can no longer be said to present the actual state of physiology. Measures consequently required to be adopted, in order to keep the work in that condition, in which it might still present to the physiological student a correct view of the actual state of the science. The method which, to the translator, appeared most eligible, was that of publishing a supplement containing all the most important accessions that had been recently made on the physiology of motion, the senses, generation, and development. In this undertaking, Dr Baly has been ably assisted by Dr William Senhouse Kirkes; and the performance produced by these gentlemen is well entitled to the attention of physiological readers, as one which presents, within moderate limits, a just view of all the new information which the progressive advancement of science has afforded. The sections on generation and development show great attention to the subject, and are extremely interesting. In short, all that can be done for the treatise of Professor Muller, Dr Baly and Dr Kirkes have accomplished in this Supplement.

It is nevertheless generally allowed by competent judges, that no work which is enlarged by supplements and appendages is so convenient and so useful as one which embraces a full view of the subject, since the same unity cannot be preserved, nor can the same balance be maintained between the different parts of the treatise. To one of the authors of the supplement this idea appeared in so clear a light, that he has carried it into effect by publishing a separate and independent work. We have already spoken of the large size and the minute details of the work of Muller; and it is unnecessary here to point out what has been matter of general observation among those conversant with students, and acquainted with their wishes and their wants. The Hand-book now published by Dr Kirkes was originally intended to present, in an abridged form, the views of physiological facts given by Muller, very much after the method of that well-informed and accurate writer. This undertaking Dr Kirkes has accomplished with great skill and ability; and the manual which he has produced is, in all respects, well calculated to serve as a guide to the student in making him acquainted with the elements of physiology.

Dr Kirkes has arranged the materials of his work in nineteen chapters.

In the first he gives a general view of the chemical composition of the human body, and the various chemical principles which it contains. In the second chapter, he gives a short sketch of the structural composition of the human body, and the various substances of which, as an organized body, it consists. In the third chapter, he mentions the vital properties of the organs and tissues of the human body.

The fourth chapter is devoted to the subject of the blood ; its coagulation ; the circumstances affecting coagulation ; the characters of the blood corpuscles or blood cells ; the characters, physical, chemical, and vital, of the serum ; the chemical composition of the blood ; and, lastly, the vital properties and actions of the blood.

In chapter fifth, the author treats of the circulation of the blood through the heart and by the action of that organ, through and by the action of the arteries, through and by the action of the capillary vessels, and through and under the influence of the veins.

In the sixth chapter is examined the function of respiration under the following heads ;—the structure of the lungs ; the movements of respiration ; the movement of the blood through the respiratory organs ; the changes which the air undergoes in respiration ; the changes effected in the blood by respiration ; the influence of the nervous system in respiration ; and the effects of suspension and arrest of respiration.

In the seventh chapter, the sources of animal heat are considered.

In the eighth chapter, the function of digestion is considered in its successive stages, in the mouth and fauces, in the stomach, and in the several divisions of the intestinal tube.

The function of absorption, lacteal and lymphatic, follows in the ninth chapter.

The tenth chapter is devoted to the subjects of nutrition and growth ; the eleventh, to that of secretion by membranes and by glands ; the twelfth, to the vascular glands ; the thirteenth, to the skin and its secretions ; and the fourteenth to the kidneys and their secretion.

Next comes the fifteenth chapter, in which the functions of the several divisions of the nervous system are considered in a degree of amplitude and detail proportioned to the difficulty, the complexity, and the importance of the subject.

In the sixteenth chapter are considered the agents, phenomena, and causes of motion in the animal body, and then successively, ciliary motion and muscular motion.

The seventeenth chapter treats of the organs of voice and speech ; and the eighteenth of the different senses.

The nineteenth chapter, which concludes the work, presents a

useful and correct summary of the present state of knowledge on the function of generation, and the process of development, from impregnation to the detachment of the young animal, with a complete and correct history of the development of the individual organs, and the successive stages through which they pass.

The value of the work is greatly enhanced by four excellent engravings, and a great number of distinct and ably-executed wood-cuts. The physiological reader will find it a most excellent guide in the study of physiology in its most advanced and perfect form. The author has shown himself capable of giving details sufficiently ample in a condensed and concentrated shape, on a science in which it is necessary at once to be correct and not lengthened.

Of the *Outlines of Physiology* by Dr Allen Thomson only two parts, or 308 pages, have been published; and it is therefore premature to give any opinion which may be applicable to the whole performance. It may nevertheless be proper to advert to the specimen of the work already published, and, from the manner in which the author has performed his duty, and his own account of what is to follow, to communicate some idea of the work.

Dr Thomson divides his *Outlines* into two parts; the first on General Physiology, and the second on Special Physiology. These parts are arranged and subdivided, so far as they go, in the following manner.

First comes an Introduction on the Nature and Objects of the Science of Physiology; in the course of which the author specifies the peculiar characters of living and animal bodies, the nature of the laws by which their actions are regulated, and the kind of knowledge required for the rational and successful cultivation of physiology.

The first part, on General Physiology, is divided into four chapters, each of which is subdivided into sections. In the first chapter is given a preliminary view of the nature and phenomena of organized bodies in general; of life; vitality; vital forces; vital principle, and similar abstractions. The second chapter presents a comparison of the vital phenomena of animals with those of plants, in the course of which the author considers shortly the structure presented by the individuals of the two kingdoms, the chemical composition, the mode of existence, and vital phenomena common both to plants and animals, and the vital phenomena peculiar to animals.

In the third chapter, the author treats of the varieties in the structure and functions of different animals, and gives under this head an arrangement of the animal kingdom into divisions, classes, orders, and sub-classes, and groups or tribes; enumerates the leading circumstances of anatomical structure proper to each; and

concludes with some observations on the position of man in the animal scale, and gives a tabular view of the different races according to the most recent authorities.

The fourth chapter consists of a general view of the minute structure and properties of the several textures composing the body of man and the higher animal tribes, and of their relation to the organic processes; and, under this head, he gives a classification of the textures, and takes a short view of the nature and origin of organized cells, the origin of *nuclei*, and the different modes in which cells are associated so as to form different organized structures; and, as the evidence on which this branch of physiological anatomy rests is entirely microscopical, he adds some judicious observations on the employment of the microscope in physiological research. These observations, though short, will be read with interest and much advantage.

These observations conclude the first part of the *Outlines*, or that on General Physiology.

The second part the author commences with a table or classification of the functions presented by animals. These the author distinguishes, much after the manner of physiological writers in general, into three great orders,—the animal functions, the nutritive or organic functions, and the reproductive functions. Under the first head are arranged in five successive chapters the subjects of, 1. Animal Motion and Mechanical Functions; 2. Nervous Action; 3. Sensation; 4. the Functions of the Brain; and, 5. the Mental Faculties. It is here to be observed, that the author has proceeded in the present two parts no farther than the end of the Proper Sensations; and consequently, in the part of the work which is yet to follow, are expected the exposition of the functions of the brain and the mental faculties, and the whole of the two following divisions.

These, as given in the introduction, are the following. The nutritive functions are divided into six;—1. Circulation of the Blood; 2. Respiration; 3. Digestion; 4. Absorption and Sanguification; 5. Secretion; and, 6. Growth and Nutrition. These are to form the subjects of an equal number of chapters.

The Reproductive Functions are divided into three parts;—1. Generation, embracing the formation and meeting of the ovum and seminal fluid; 2. Uterogestation or Nidification; and, 3. Embryology, or the development and life of the *fœtus*.

The *Outlines of Physiology*, so far as they go, are composed with great ability. The statements are given in clear and precise terms; the views and generalizations are philosophical; and everywhere the author shows that familiarity with the subject which all who know his diligence and talents are prepared to expect.

It is not our intention to examine in detail the contents of these and the other writings, the titles of which are placed together be-

fore our readers. But it may be useful to advert to several points in the recent progress and present state of physiological science.

It is unnecessary to recal here the object and nature of that science. Though the term was originally applied to the study of nature and the material world in general, it is long since that vague and comprehensive generalization was abandoned. In modern times, and by modern writers, the term has been confined to signify the knowledge of the nature and causes of the phenomena presented by living bodies,—bodies, that is to say, possessing life,—to the study of the actions taking place in these living bodies simultaneously and successively, and to the knowledge of the laws by which these actions are regulated.

This definition leads naturally to the question, what bodies are living, and what is life? and on this point enough has been said in the previous article. It only remains in the present article to advert to a few important points on the characters and phenomena of living bodies.

One of the great leading characters of living bodies is their power of applying or receiving into their own substance foreign matters of various kinds; and this character applies both to their first origin and to their whole duration. When the germ or ovum is impregnated in the parent animal, though previously it had continued in a dormant, passive, or inert state, yet no sooner is it impregnated than it becomes the seat of a new set of actions. New matter, in the form of blood, is carried to it; vessels are formed in it; and these vessels convey and apply the materials thus brought in order to enlarge its size and make it grow. At this period, the new matter is brought directly to its vessels and deposited in them. But, after a certain time, when the newly-formed ovum is detached from the parent, a new mode of nutrition is employed. The foreign materials, instead of being conveyed directly into the blood-vessels, are deposited in the stomach, a sac or cavity communicating with the exterior of the animal; and in that sac and its appendages the materials or aliments undergo certain preliminary changes in successive stages, before they can be received into the sanguiferous system. Even after their reception into the vessels of that system, they require to be subjected in a peculiar manner to the action of the atmospherical air before they can be applied to the repair and enlargement of the various organs and tissues of the body.

This power, therefore, of appropriation appears to be one of the essential characteristics of life.

Another essential character of living bodies is peculiar arrangement of component particles,—in other words, definite structure and organisation.

This has been shown, by microscopical examination of the tis-

sues of plants and animals, to be either universally or very generally cellular, that is, a series or arrangement of cells. These cells are further distinguished by containing an internal body or nucleus, which is not placed in the centre, but generally at a little distance from that centre.

We have formerly said that Hewson may be regarded as the first person who made any approach to the discovery of the cellular structure. His demonstration of the constitution of the blood-vesicles may be regarded as the first step in this inquiry. Subsequently this formation was discovered in the vegetable world by the distinguished botanist, Robert Brown. Long afterwards, namely, in 1838, Schleiden showed that many of the textures of plants could be resolved into this cellular elementary arrangement; and, almost immediately afterwards, Schwann published his observations, showing that several of the textures of the animal body were, in the same manner, formed from cells.

Dr Martin Barry, it was shown in our fifty-fifth volume, in 1841, has the merit of tracing the nascent animal from this original structure; and his subsequent researches have tended to throw further light on the universal distribution of this structure in the vegetable and animal world.

Many inquirers have engaged in this investigation since the first attempts by the observers now mentioned; and each has more or less contributed to extend and render accurate the history of this theory of formation by cells. A very able digest of the facts ascertained by the principal inquirers is given by Dr Baly in his Supplement; and this it may be proper to place before the reader, both from its instructive character and as a specimen of the manner in which the authors of that work have performed their duty:—

“1. Very little requires to be said concerning the *composition of the cell* itself. Its membrane or wall appears, by almost universal assent, to be formed of a protein-compound, most probably of albumen, except in a few cases in which it seems to be composed of a substance more allied to fibrine. Although the cell-wall is rendered transparent and indistinct by acetic acid, yet it is not dissolved by this reagent, as is usually supposed to be the case, for, on the addition of an alkali, such as a solution of potash or ammonia, its form and other external characters are in many cases restored. With respect to the *contents* of cells, it is perhaps sufficient to state that further investigations continue to show how various these may be, the varieties being as numerous as the functions which the cells discharge, and often differing in the same cell at different periods of its life. These contents, although occasionally composed of a clear fluid of various consistency and colour, are usually more or less granular, the granules consisting of different colouring matters, of

fat particles, and of a fine molecular substance, whose nature is still obscure.

"There is much discrepancy in the accounts given by different writers concerning the composition and general characters of the *nucleus*. This discrepancy is probably in great measure due to the fact that, after their formation, the nuclei undergo various alterations in aspect, if not in composition, and in some measure also to the fact of there probably being some original differences in the nuclei of different cells. Sometimes the nucleus occurs as a more or less solid body of a granular aspect, while at other times it appears as a pale vesicle with a distinct cell-wall and fluid contents. And between these two conditions varieties are occasionally found which would seem to prove that the one is only a modification of the other, and that these several varieties represent so many transitional stages between the two. The pale vesicular form is by far the most general one, and, in Kölliker's opinion, is the constant form of the nucleus in the early stages of the cell's life. It has been long known that the nucleus has a different composition to that of the cell, many agents which act upon the one having no effect upon the other. Kölliker is of opinion that the membrane of the vesicular nucleus is composed of pyin, the clear contents of albumen, and the nucleolus of fat. His opinion that the membrane is composed of pyin is derived chiefly from the fact of the nucleus being insoluble in acetic acid, a property which is possessed by no other nitrogenous compound except chondrine; and this substance is soluble in the gastric fluid, while the nuclei are not, neither is pyin. The presence of albumen in the contents of the vesicular nuclei he thinks is proved by the contents of the germinal vesicle (which he considers to correspond to the so-called nucleus of other cells) being rendered granular by ether. The fatty nature of the nucleolus is indicated by its aspect, and by the presence of fat in parts composed chiefly of cells.

"Schleiden described the nucleus in the cells of plants as being invariably situated within the substance of the cell-wall, which at that point divides into two laminæ, between which the nucleus is placed. In animals also the nucleus is commonly situated at the wall of the cell, sometimes apparently imbedded in its substance, but, according to Schwann, most frequently attached to its inner surface, and never invested internally by a layer of the cell-wall as it is in plants, according to Schleiden. Henle states that sometimes, as in pigment-cells and the cells of the crystalline lens, the nucleus is situated outside the cell-wall, which at that part presents a shallow depression to receive it; but Dr Sharpey is inclined to doubt the exterior position of the nucleus in these cases. Occasionally the nucleus is situated towards the centre of the cell, as is well shown in the cells of cylinder-epithelium. In such cases, however, the nucleus does not usually appear to lie free in the cavity of the cell, and to admit of being altered in position as the cell rolls over, but it seems to be quite fixed, and probably adheres to the internal surface of the cell-wall which, in cylindrical cells,

closely surrounds it in one plane, and in flat cells is in contact with it at opposite sides. It was stated by Hewson, however, that when the nucleated blood-corpuscles of fish or reptiles are swollen with water, and watched when rolling over, the nucleus may be distinctly seen to fall from side to side in each distended corpuscle; and Schultz appears to have recently advanced a similar opinion. But Henle remarks that he has never been able to witness this phenomenon, and he considers the nucleus of blood-corpuscles, as also those of mucus-corpuscles and epithelium-cells, to be attached to the inner surface of the cell-wall.

"The nature and composition of the *nucleoli*, or nucleus-corpuscles, still remain obscure. Henle is doubtful whether what have been described as nucleoli may not be merely spaces in the interior of the nucleus. He thinks that this view of their nature is supported by the circumstance of no apparent chemical difference being perceived between them and the nuclei; agents which destroy the one invariably destroying the other also. Vogt, although he admits the real existence of nucleoli, usually of a vesicular character, yet agrees with Henle in regarding them as unessential elements of a cell. He states that when they appear, it is only at a late period of the cell's life, and that shortly after their formation they usually assume a vesicular character, and as they enlarge are probably developed into cells at the expense of the nucleus which they gradually destroy. Kölliker, however, entertains an entirely different view of the nature and importance of the nucleoli. In his opinion, the nucleus ought to be regarded as a primary nucleated cell, and the structure usually called a primary cell as a secondary cell. In the formation of such primary cell (the nucleus of other writers), he believes that a round, dark, apparently homogeneous substance is first developed in the formative fluid. Around this body, which by him is regarded as the nucleus, by others as the nucleolus, the wall of the primary cell is gradually developed. Occasionally two, more rarely three, and still more rarely four, dark particles are found in a single primary cell (nucleus). Whatever may be the number, one at least is invariably found in every such cell up to a certain period of its growth. When single, the particle is situated on the wall of the cell; when there are several particles, they may occupy a similar situation, or be free in the cavity of the cell. Occasionally one or two particles apparently identical with these are found also among the contents of the secondary cells (or primary cells of other writers). They have all the appearance of being composed of oil or fat. Indeed, they appear to be identical with the elementary granules commonly found in the cytoblastema, and which Henle (as well as others) describes as minute vesicular-looking particles of fat. And it is difficult to determine in what respect they differ, and why Henle should discard the use of the term nucleoli; for, as will be presently shown, he admits the importance of the elementary granules in the first formation of cells. Kölliker confirms Vogt's statement that the nucleoli are sometimes developed into vesicles, which then enlarge, apparently at the expense of the



nucleus, which disappears when these vesicles have attained a certain size, the vesicles themselves likewise disappearing soon afterwards. But in other cases, as will be presently shown, the nucleolus, instead of disappearing, becomes constricted in the middle, and subsequently divided into two equal portions, around each of which a new cellular body or nucleus is then developed. In all those cases, however, in which the nucleus or cell undergoes transformation into a higher tissue, the nucleolus disappears. But this is certainly not invariably the case, for, in the persistent nuclei of capillary blood-vessels, of the sarcolemma, and of several other tissues, a small dark particle, apparently identical with the nucleolus, may usually be observed.

"2. In considering the various modes in which the *development of cells* in the formative fluid or cytoblastema is effected, it must be remarked, in the first place, that it appears immaterial to the process in what part the formative fluid is situated. The same succession of changes in the formation of cells seems to be pursued whether the process occurs in the cytoblastema of the early ovum, in the secondary cytoblastema from which the several embryonic tissues are produced, or in the organizable material effused from the blood-vessels into the interstices of the various parts of the growing or adult body. The fluid in each case appears to possess the same formative properties, and the chief or only difference observed in the process relates to the mode in which the cells are ultimately disposed of. In the increase, also, of cells, by endogenous multiplication, the formative fluid out of which the young cells grow, so far as concerns its power of producing new cells, appears to be essentially the same as the cytoblastema elsewhere: and the differences in the mode of growth are probably more apparent than real, the developing cells in the one case lying free in the interstices of parts, in the other case being enclosed within a membranous envelope or parent-cell.

"In the opinion of Schwann, the development of cells pursues an almost exactly similar course in every case; and he believes that the subsequent multiplication of animal cells is usually effected by the same series of changes as are undergone in their original development, the endogenous mode of origin so common in vegetable structures being rarely pursued in animal tissues. But, as will be presently shown, the result of more recent investigations has made it probable that this mode of origin, or rather of multiplication, is of more frequent occurrence than Schwann supposed. The plan of cell development recognised by Schwann is detailed in Professor Müller's *Elements of Physiology*. In addition to this, the ordinary mode of development, Schwann also suggested the probable occurrence of a variety or modification of it in some cases. For having observed that occasionally the nucleus of a cell contained two nucleoli, he thought that the circumstance might be explained by conceiving that two (or more) contiguous nucleoli, with their layer of granular deposit, had fused together before either of them had attained such a stage of development as singly to constitute a nucleus. And in those cases in which the nucleus of a cell appears

to consist of two or more portions, he inferred that the component parts were so many nuclei which had been contiguous to each other, and fused together before the growth of the cell-wall around each had made much progress.

“According to Henle, the formation of cells takes place in three different ways ; in two of these (which appear to be only modifications of each other) the nucleus is developed first, while in the third it is not formed until after the cell, or even does not appear at all. In whichever of these ways the cells are developed, numerous spherical or oval fat-like particles first make their appearance in the cytoblastema or formative fluid. In one of the three modes of development, a layer of the dimly-granular material of the cytoblastema appears to deposit itself upon one of these fat-like particles, and thus to form a nucleus, upon which a cell-wall then grows, though, as will be noticed again presently, in a manner somewhat different from that pointed out by Schleiden and Schwann. In another mode, the nucleus is formed by the grouping together and coalescence of two, three, or even four of the elementary particles ; a cell-wall is developed around this compound nucleus in the same manner as around the simple one. As the growth of the cell proceeds, the component particles of the nucleus become completely fused together, and a single smooth body eventually results. The compound nature of the nucleus of epithelial cells, and of pus-corpuscles, after being acted upon by water or dilute acetic acid, is by Henle attributed to the fact of such cells being examined at an early period of their growth, and previous to the complete coalescence of the several particles composing the nucleus. Henle believes that this mode of development prevails among most elementary cells of the animal body, and he refers, in illustration of it, to the corpuscles of mucus and pus, to those of the lymph and chyle, and to the cells of most glandular structures. With regard to the mode of production of the cell-wall around the nucleus, Henle is of opinion, with other physiologists, that the several elementary granules are so many particles of fat, and that around each one, or a group of them (according as the nucleus happens to be simple or compound), a layer of the albuminous matter of the cytoblastema coagulates and forms a kind of film or coating, in accordance with the fact pointed out by Ascherson, that minute globules of oil, when brought into contact with liquid albumen, become at once invested by a coherent layer of the albuminous substance, and thus acquire a vesicular character.

“On comparing the above two modes of cell-development with the account furnished by Schwann, it will be observed that there is no striking difference between them ; the first plan of development described by Henle agrees essentially with that stated by Schwann, while between Henle's second plan and Schwann's explanation of the origin of the cells containing two (or more) nucleoli, the difference is more apparent than real, and is not in either case founded on direct observation. The chief discrepancy in the accounts of these two observers appears to consist first in Henle's disinclination

to admit the existence and importance of nucleoli, though, as before observed, there is no good reason for regarding the nucleoli as structures dissimilar from Henle's elementary particles or granules; and, secondly, in respect of the manner in which the cell-wall is developed around the nucleus.

"In Henle's third mode of the development of cells, a large quantity of the elementary granules arrange themselves together into a more or less spherical mass, around which a delicate cell-wall is subsequently formed; but it is not until a later period, if at all, that a nucleus can be perceived in the midst of this mass. Illustrations of this mode of development are presented by the large granular bodies met with in the first milk or colostrum, by the so-called compound inflammation- or exudation-corpuscles, and by many of the globules found in malignant tumours and other morbid products.

"Besides these three modes of cell-development, however, Henle recognises, with Schwann and other physiologists, another plan in which simple cells are developed, independent of a pre-existing nucleus. Examples of this are seen in the *chorda dorsalis* of fish and reptiles, as Schwann pointed out, and in cryptogamic and many higher plants, in which a single minute spherule first appears, and this soon becomes a distinct vesicle, rapidly grows, and is eventually extended into a cell.

"The results of investigations by Vogt, on the development of fishes and reptiles, also tend to show the occurrence of at least three distinct forms in which the development of cells may take place. In one of these forms the cells appear to owe their origin to a pre-existing nucleus, but in the two others they appear to originate independently of a nucleus.

"As already stated, Vogt entirely agrees with Henle in his view of the unimportance of the nucleolus in the process of cell-formation. In by far the majority of cells in young Batrachians and fishes nucleoli were entirely absent; and in the few in which they existed, as the cartilage-cells of Batrachians, and the embryonic-cells of the salmon, they appeared to be structures of later formation, occurring as simple vesicles which gradually enlarged into cells apparently at the expense of the nucleus, which by degrees entirely disappeared. In no case did they appear to constitute the first stage in the development of cells out of the cytotblastema in the manner described by Schleiden and Schwann. The nucleus, however, appears to be an almost invariable constituent of the cell at whatever period of its life it be examined. But the relation, in point of time, which its development bears to the development of the cell, was found by Vogt, as by Henle and others, to vary in different cases. In one form of cell-development, namely, the production of the cortical cells of the yolk in the toad, the nucleus precedes, and evidently gives rise to, the formation of the cell. In another form, which, as shown by Henle, is well illustrated in the *chorda dorsalis* of fish, the cells originate without the intervention of nuclei, which only make their appearance after the cells are fully formed. In the

third form, the cell and its nucleus seem to be developed coincidentally. For, in the embryonic cartilage of the toad, in which this mode of development occurs, Vogt never could detect either free nuclei or cells unprovided with nuclei; when nuclei were found, they were invariably surrounded by a cell-wall; and when cells were found, they invariably enclosed a nucleus. In explanation of this coincident formation of cell-wall and nucleus, Vogt suggests that probably one portion of the granular matter of the cytotblastema, from which a cell is about to be developed, may collect centripetally at the centre, to form a nucleus, while another portion may collect around, at some distance from it, by a centrifugal influence, and there form a cell-wall.

“Kölliker's opinion of the mode of origin of cells, founded upon the results of researches on the development of invertebrate animals, differs in several respects from those entertained by Henle and Vogt. For he believes that the so-called primary cell is, as Schleiden and Schwann described, in almost all cases developed around a nucleus, which persists for a greater or less length of time, and that the nucleus also is in most cases formed around a nucleolus. The irregular appearance frequently presented by the nucleus of pus-corpuscles, especially after being acted upon by dilute acetic acid, is not, as Henle supposed, an early character, and an indication of its being originally composed of two or more separate particles—for, at its first formation, the nucleus is invariably a simple vesicular body,—but is an after effect, and is due to the nucleus being divided into two or more new vesicular bodies, each of which may, if carefully examined, be seen to contain a minute particle or nucleolus; and these, he thinks, originate by endogenous multiplication.

“In a recent essay, H. Müller has advanced an opinion concerning the development of the corpuscles of pus and of chyle, which differs from that of other writers, and from which it would seem that these corpuscles originate in a manner somewhat similar to the third mode of development described by Vogt. He believes that previous to the development of cells the chyle consists of a number of particles, some of which are soluble, others insoluble. In the production of chyle-corpuscles or cells, a number of both kinds of particles become aggregated into a mass; shortly after the formation of which, the insoluble particles collect together in the centre to form the nucleus, while the soluble ones dispose themselves around the circumference, and are transformed into the cell-wall. A very similar process he states to be pursued in the formation of pus-corpuscles.

“Such are some of the principal observations which have been lately made on the subject of the development of cells. The amount might have been considerably extended by including the remarks of many other writers on the subject; but since these have, for the most part, a tendency to confirm one or other of the views stated above, it is perhaps unnecessary to do more than refer to them here. In collecting together the above facts, it has been the writer's endeavour to ascertain whether the various accounts of different observers could be so far reconciled as to furnish conclusions pointing

to the existence of any one uniform and constant plan, according to which the development of cells is in all cases effected. But it will be at once evident, from what has been stated above, that so far as our present data extend, no such single uniform plan can be said to exist; though it is not improbable that further investigations will shortly lead to its discovery, and that then the several varieties hitherto observed may be found to be only modifications of one universal mode of development.

"From what has been said above, it appears tolerably certain that cells may sometimes originate independent of pre-existing nuclei, and that, therefore, the views of Schleiden and Schwann in respect of the importance of these structures in the genesis of cells, must be somewhat modified. Yet it is not satisfactorily shown that in any mode of cell-formation cases ever occur in which one or more minute elementary particles, corresponding to the nucleoli of Schwann, do not exist previous to the formation of any other part of the cell. If subsequent investigations prove that the pre-existence of such particles is a circumstance of invariable occurrence, it may be reasonably inferred that they are the real germs or cytoblasts from which the cells originate. When once formed, these particles may give rise to the production of cells in one or other of the various ways above described. Each one may either grow and be itself developed into a cell by incorporating nutritive matter, and simply enlarging, as is supposed by Mr Macleod to be the case in the development of the blood-corpuscles of the chick, by Vogt in the development of the cells of the *chorda dorsalis*, and by Karsten in the development of all varieties of cells. Or it may serve as a centre around which matter is deposited to form a nucleus, from which a cell-membrane subsequently springs, in the manner maintained by Schleiden and Schwann, to prevail in most vegetable and animal tissues. Or, again, it may serve as the true nucleus to a primary cell growing around it; and this, by Kölliker, is considered to be its ordinary office. It must be mentioned, also, that even a primary cell may act the part of a nucleus, so far, at least, as to cause the growth around it of another secondary cell-membrane. Examples of this are furnished by the ganglion-corpuscles of nerve-substance, and by the ovum. Kölliker, indeed, considers that all ordinary nucleated cells should be regarded in the light of secondary or complex cells.

"3. From the several details which have just been considered in relation to the development of cells, it would appear that in the cytoblastema there resides some power by which fresh cells can be continually formed out of an apparently homogeneous fluid. In order that this continual formation of successive crops of cells may be effected, it is essential, however, that constant supplies of new formative fluid should be provided, and it appears to be one of the purposes served by cells to elaborate this fresh formative material, which, when perfected, is discharged by the solution of the membranous cell-walls. Out of the fresh cytoblastema, thus prepared and liberated, the new cells are developed in one or other of the

ways above pointed out. And it would seem, as stated by Schwann, that, in the case of animal structures, the continued increase of cells is, in most cases, effected by such fresh development in the free formative fluid. But in several other cases new cells are formed within the cells of a preceding generation, and by these they are surrounded until they have attained a certain degree of development, when they escape, apparently by rupturing the parent cell, which then disappears. This *endogenous* mode of cell-formation (or *multiplication*, as it is commonly termed), although of common occurrence among vegetable structures, is, however, comparatively rare in animals; the ovum, cartilage, and a few other structures presenting the only known examples of it. It differs from the original development of cells in the circumstance of the new cells being produced more or less directly from some part of a pre-existing cell, which thus acts as a kind of reproductive organ. But it is not improbable that the difference is one more apparent than real, and consists simply in the circumstance of the source whence the new cells originate being in the one case retained within the parent cell, and in the other case set free.

“The best examples of the endogenous mode of cell-multiplication have been already mentioned in describing the changes which ensue in the development of the ovum. It was there shown (in the case of the ovum of *Cucullanus elegans*) that, according to Kölliker's observation, the first step in the process of multiplication consists in the nucleus of the first cell which is formed after the disappearance of the germinal vesicle becoming constricted in the middle, and subsequently dividing into two equal halves, each of which serves as a separate nucleus, around which a new cell forms; and each new cell in its turn gives rise to two others formed in the same way, and so the process goes on until the whole mass of the ovum is made up of such cells. And Kölliker appears to be of opinion that in most other cases of cell-multiplication the division of the nucleus is the first essential step in the process. Other cases, however, seem to occur, in which the nucleus, instead of dividing into two portions, only breaks up into several particles (though even this may be effected by successive duplications), each of which appears to possess the power of enlarging and becoming vesicular; the several minute vesicles, as they increase in size, gradually obliterating the original nucleus, and eventually constituting the chief contents of the cell. Each of these minute vesicular particles probably constitutes a germ of a fresh cell, into which it is subsequently developed, either by simple enlargement, or by serving as a cytoblast around which a cell-wall forms. In other cases, again, apparently under the influence of the nucleus when present, or even independent of it, minute vesicular bodies are developed within the cell itself, which by enlarging they gradually fill, and eventually burst. Previous to their discharge from the parent cell, or shortly afterwards, a new generation of cells is developed within each of them by the same process by which they themselves had been formed. Another form of cell-multiplication has been described as occurring in vegetable struc-

tures, in which a cell appears to divide by the formation of a partition across its cavity, whereby two new cells are formed. But, as explained by Schleiden, in which explanation Dr Sharpey agrees, this apparent mode of division is probably merely an instance of the endogenous production of twin cells, the contiguous sides of which form the septum. In a few cases, again, the multiplication of cells takes place by the growth of young sprouts or offshoots from the parent cell. This variety, which is confined entirely to vegetable structures, is well illustrated in the mode of growth of the yeast-plant.

"4. The tendency of nearly all recent observations on the subject has been to confirm the general correctness of Schwann's account of the various *changes which the primary cells undergo in the production of the elementary tissues* of the body. In some instances, however, there is sufficient evidence to show that this account requires to be modified. This seems to be especially the case in regard to the cellular, tendinous, and elastic tissues, each of which was supposed by Schwann to be formed by the elongation of cells and their division into bundles of fibres. But that such a mode of development appears not to take place will be presently shown when considering the transformations undergone by the nuclei.

"In regard to the development of bone, a considerable amount of information has been of late added to the comparatively imperfect account of it furnished by Schwann. But it is considered unnecessary to enter here into the details of this, since the whole of the subject has been of late so ably discussed by Dr Sharpey, in a standard work on anatomy.

"Several new facts have also been added concerning the development of nerves, which tend to throw fresh light on the physiology of the nervous system, since they render it almost certain that the central terminations (or origin) of nerve-fibres are not disposed in loops, as until lately has been generally supposed to be the case, but that they pass directly into the nerve-corpuscles, which compose so large a portion of the grey substance of nervous centres. Both Müller and Remak, several years ago, observed that from some of the corpuscles of the grey substance of the brain, spinal chord, and ganglia, fine tooth-like processes issue, and may be sometimes traced to the extent of many times the diameter of the corpuscles. The resemblance which these processes bear to the delicate grey filaments observed by Remak in the sympathetic nerves, led to the suggestion that the two are identical, and that the latter filaments take their origin directly from the ganglion corpuscles. These observations, however, do not appear to have attracted much further notice; but it has been found by more recent investigations, that Remak's suggestion concerning the origin of sympathetic nerve-fibres is perfectly correct, and, moreover, that the fibres of the cerebro-spinal nerves also have, as was indicated by Ehrenberg, an exactly similar origin. Without entering into the details of these important investigations, the consideration of which would be foreign to the present purpose, it may be remarked that, in the junc-

tion of the nerve-fibres with the ganglion-corpuscles, the contents of the central part of the fibre (the axis-cylinder of Purkinje and Rosenthal, the primitive band of Remak) pass directly into the granular contents of the corpuscle, while the fine external sheath of the nerve-fibre becomes continuous with the membranous envelope, within which the granular substance of the corpuscle is contained. The phenomena observed during the development of nerve-fibres in the embryo, especially by Schaffner and Kölliker, agree very closely with these facts. In the earliest period of its formation nerve-substance consists almost entirely of roundish, mostly nucleated cells, filled with a finely-granular material, and, with the exception of being somewhat smaller, exactly similar to the nerve-corpuscles found in the nervous centres of the adult animal. As the development proceeds, but previous to the appearance of distinct nerve-fibres, many of these cells send forth fine tubular processes of an apparently homogeneous structure, which unite with similar processes from other cells, and thus, in time, give rise to continuous nerve-tubules. Kölliker finds that, in young Batrachians, a complete network of nerve-tubes is formed by this junction and coalescence of the processes from branching cells: a similar observation was also made by Schwann. According to Schaffner, as the nerve-tubules coalesce and increase in size, the walls of the cells from which they originate are gradually drawn out and merge into those of the tubules, while their granular contents also become continuous and identified with the contents of the tubules.

“In considering the transformation which cells undergo in the development of tissues, too much stress cannot be laid on the importance of the share taken by the nuclei in these changes, especially since this appears to have been entirely overlooked by Schwann. It is proposed, therefore, to bring together some of the more striking circumstances which seem to demonstrate the importance of nuclei, whether considered as individual structures, or as component parts of cells. That the nuclei may exist in tissues apparently independent of cells, has been shown, especially by the observations of Mr Paget, who found that many morbid growths are composed almost entirely of corpuscles like nuclei or cytoblasts. These morbid structures were usually tumours of very rapid growth, and, from the almost invariable presence of large quantities of nuclei, it would seem that they must play an important, if not the chief part in this growth. The abundance of nuclei in most, if not all, other actively growing tissues, healthy as well as morbid, their persistence in those tissues, such as the muscular, in which a constant waste and repair consequent on the active discharge of their function is taking place, their invariable existence in the secreting cells of all glands and epithelia, and their disappearance from the cells of fat, which when fully formed cease to perform any active function,—all attest the importance of the share taken by the nuclei in the processes of growth, reproduction, and secretion. Equally strong confirmation of this is furnished also by the variety of examples in which development, in either structure or composition, is effected in the animal organism



by cells unprovided with nuclei, while there are many instances in which nuclei, whether contained in cells or without them, appear to assume higher forms, or to be centres and sources of formative and reproductive power. The evidence of these facts is based chiefly on his own observations on tumours above alluded to, and on the investigations of Professors Henle and Goodsir, and of Mr Simon. The researches of the last named observer on the glands without ducts tend to prove the discharge of a large amount of gland-function by nuclei alone; for, in the thymus, the spleen, and other such glandular organs, minute vesicular bodies, in all respects similar to nuclei or cytoblasts, exist in considerable abundance, and appear to be the essential parts concerned in discharging the functions of these organs. And Professor Goodsir's observations in several of his papers seem to demonstrate the power of the nucleus both in the production and multiplication of cells, and in the formation and storing of secretions.

"The transformation of nuclei into higher tissues has been shown, especially by the researches of Henle, and more recently by those of Kölliker. According to Schwann's system of cell-formation, the nucleus is supposed to disappear shortly after the perfect state of the cell is attained. But the results of recent observations have shown that the disappearance of the nucleus is of much more rare occurrence than was supposed by Schwann to be the case, and, moreover, that instead of disappearing, the nucleus in many cases assumes a higher degree of development, and is transformed into a more or less persistent tissue. According to Henle, the only parts in which the nucleus disappears are the blood-corpuscles, the cells of the epidermis and the nails, and most of the fat-cells, the tubules of the crystalline lens and of enamel, and many of the cartilage-corpuscles. But, in all fibres supposed to be formed from coalescing cells (except those of the lens and enamel), the nuclei remain, and, moreover, undergo remarkable transformations. For example, they assume an oval shape, then, gradually elongating and becoming narrow, are converted into fine dark streaks, which lie in straight, angular, tortuous, or spiral lines upon the fibres. After being thus changed, they either gradually disappear, or, becoming more elongated and meeting with each other, they unite to form a new set of fibres, which, from their mode of origin, he calls nucleus-fibres. Occasionally these nucleus-fibres send off lateral branches, by which a kind of continuous network is formed over the surface of each layer of the tissue in which this arrangement occurs. Various other modes of arrangement of these nucleus-fibres are observed in different tissues. The fibres are remarkable for their dark well-defined outline; and being insoluble, like other nuclei, in acetic acid, their existence and peculiarity in a tissue may be at once ascertained by means of this re-agent. Ordinary elastic tissue appears, according to Henle, to be only a variety of such nucleus-fibres.

"Another remarkable purpose served by nuclei in the formation of tissues has been pointed out by Henle as seen especially in the coats of blood-vessels. In the development of these coats, layer

after layer of cytoblastema is deposited in the form of structureless membrane, and, in each of these, nuclei are shortly formed and undergo several different changes. In the innermost layer cells grow around the nuclei, and thus is formed the epithelial coat of the vessel. In the next layer, which forms the so-called internal coat of the vessel, the nuclei remain unaltered. But, in the formation of the muscular or contractile coat of arteries, the nuclei elongate and arrange themselves in rows in the manner before described. Moreover, each row of elongated nuclei appears to appropriate the adjoining strip of structureless membrane in which it is imbedded, and the result is that this membrane breaks up into a number of flat fibres, each bearing upon its surface the row of nuclei after which it was modelled. Organic muscular fibres of other parts of the body are formed after exactly the same plan. In the formation, also, of fibro-cellular or areolar tissue, the nuclei are arranged in rows, to each of which is appropriated a strip of the cytoblastema; and each such strip, instead of remaining flat and ribbon-like, as is the case in organic muscular fibre, breaks up into a bundle of parallel longitudinal fibrillæ. This is quite opposed to the account given by Schwann of the development of fibro-cellular tissue. Kölliker, in alluding to these several transformations undergone by the nuclei, mentions also, as other instances, the different modes of development of seminal filaments directly from nuclei, and the growth of the spines of several invertebrate animals.

“Arguments in favour of the view of the importance of the nuclei to the growth and well-being of the tissues in which they occur, are furnished also by the phenomena which attend their retrograde, as well as their advancing transformations—their degradation as well as their development. For it has been rendered highly probable, by the investigations of Mr Paget, that, in all cases of atrophy accompanied with degeneration of tissue, the nuclei of the degenerated part lose their characteristic properties, or entirely disappear. This is especially the case in fatty degeneration (or atrophy) of muscle, of the liver, and of the kidney, in all well-marked instances of which the nuclei of the fibres in one case, of the hepatic and renal cells in the other cases, have completely disappeared, their place being occupied with fat, in the form of granular matter, or drops of oil.”—Pp. 113–127.

The researches of Dr Barry lead him not in all points to agree with the representations of this matter given by Schleiden. It is known that this botanist gave to the *nucleus* or opaque spot the name of Cytoblast, from its supposed property of giving origin to the membranous wall of the cell. In the cytoblast he saw what seemed a smaller body, the *nucleolus*; and Schleiden, as well as Schwann, Muller, Valentin, and Henle, supposed the small body to exist first; the substance of the cytoblast or large body they believed to be deposited round the smaller one; and the substance of the cell-membrane to be deposited round the cytoblast.

From multiplied researches made on this subject with great care, Dr Barry feels it necessary to differ from these observers, and considers himself entitled to establish a series of inferences which, in truth, give a view almost diametrically opposite to that given by the German anatomists. These inferences are,—

1. That, in the early stages of its formation, the cytoblast of Schleiden has no *nucleolus*, but acquires a *nucleolus* before its formation is completed.
2. That, when the cytoblast has formed a cell, the *nucleus* of this cell is what has been called the *nucleolus* of the cytoblast.
3. That, at this time, another nucleolus comes into view in the situation which had been occupied by its predecessor.
4. That, as this process is continued, the nucleus of one stage is the nucleolus of an earlier stage; there being a continued succession of *nucleoli* in the centre.
5. That this process begins before the complete formation of Schleiden's cytoblast; is commenced, indeed, with the existence of the minute pellucid globule, which thus becomes transformed into that cytoblast.
- And, 6. That this process is the opposite of that described by Valentin in his statement of the united views of Schleiden, Schwann, Muller, Henle, and himself, as forming the elements of the tissues.

The question of cell-formation is intimately connected with that of the development of the ovum; and it was in studying the latter process in its several stages that Dr Barry was led to form distinct ideas of the nature of the rudimental or elementary cell, its mode of formation, and the several changes which it undergoes. In the present essay, he recalls various facts established in his first researches; and compares those which he ascertained regarding the germinal vesicle to those which he has since observed in the rudimental, vegetable, and animal cell.

Dr Barry's former researches show that the essential part of the ovum is the germinal vesicle or cell; and particularly its *nucleus*, namely, the germinal spot. The first changes connected with fecundation, which are discernible in this spot, in the ovum of the rabbit seems to be of a preparatory nature; for they begin *ante coitum*, while the animal is in the state of heat. The introduction of a substance from the seminal fluid into the hyaline or centre of the germinal spot at length accomplishes fecundation of the ovum. The germinal spot or nucleus then passes to the centre of its cell or vesicle; and the contents of the cell, no longer in communication with the exterior by a parietal nucleus, undergo changes, having sole or especial reference to that fecundated centre. The concentric layers of cells Dr Barry represents to be successively and rapidly dissolved, and with equal speed succeeded by fresh broods of cells arising in the centre. The cells in the concentric layers are themselves filled with other cells, undergoing and about to undergo like changes.

After this process of dissolution and successive reproduction has continued for a certain time, that which has become the *nucleus* is divided into two halves ; which two halves, by appropriating the result of this cell-formation, and by the same process as that already described, are enabled to give origin to two cells, intended to succeed the parent.

The centre of the germinal spot is, in truth, the initial point from which proceed all the changes in the ovum. It is the source of new vital action destined to animate the new animal.

Dr Barry is led to advert to the process of subdivision into halves, or what is named cleavage,—a process which is described fully and accurately by Dr Baly, and of which a short sketch is given by Dr Kirkes. The general result is, that the nucleus is first, as already stated, divided into two portions, nearly equal, or cells which are the foundation of the new being ; that the nucleus of each of these two cells is subdivided into, or gives origin to other two, so that four cells constitute the next stage of formation ; then the *nuclei* of these four are, in like manner, divided each into two, so that eight cells constitute the ensuing stage of formation ; and a continuation of the same process effects a doubling of the cells until there is formed a mulberry-like body consisting entirely of cells. In the centre of this mulberry-like body is found a cell having the same general appearance as the others, but larger in size ; and the nucleus of this large central cell is the rudimental embryo. This embryo nucleus, undergoes in its further development, changes of the same kind as those presented by the nucleus of every cell subsequently developed from it, and entering into the formation of the embryo.

Dr Barry deduces the inference, that this successive formation of cells is a character transmitted, in every instance, from the parents to the offspring.

Dr Barry observed, in 1840, in the germinal vesicle, an orifice connecting the nucleolus with the exterior of the cytotblast and cell. Dr Harless, while at Trieste in 1846, in examining the cells of the ganglion globules in the *lobus electricus* of the *torpedo Galvani*, found nervous filaments connected with what he terms the *nucleus* of an inner cell. Dr Barry thinks, apparently with reason, that this nucleus of the inner cell is the orifice connecting the nucleolus and nucleus.

This enlargement, by division and multiplication of cells, is probably one of the most distinct and best examples of the effects of what has been believed by many to be vital force, and the influence of the vital principle. The regular and uniform formation of bodies, all upon the same model, of the same figure and structure, and all proceeding from one single cell, or rather from its nucleus, (nay, it may be said, according to Dr Barry, from one

minute point, namely, the hyaline,) must be allowed to be the strongest proof of the existence and operation of such a force or power, as that to which the characters of the vital force have been ascribed. It is also to be remembered that the ovum of the female, or rather the germinal vesicle, is in a passive and dormant state, and would remain in that state, unless in some mode acted on by the seminal liquid of the male. Without this, indeed, it would remain in this dormant state until it were altogether lifeless. The influence of the seminal liquid is the agent which gives rise to the changes now mentioned; and, when once commenced, these proceed regularly, unless checked by some force of a different kind. All these phenomena, the defenders of the hypothesis of the vital principle may most justly adduce as strong proofs of the reality and truth of that hypothesis, and of the existence and operation of the vital principle. It only remains for consideration, whether the assumption of this principle be requisite to account for the phenomena, or whether the occurrence of all these phenomena, in the manner described, may not, with equal justice, be referred to the will and wisdom of that Being by whom all animal bodies have been created. In both hypotheses, the phenomena are regular and uniform; means are employed to accomplish certain ends, and the ends are accomplished. In ordinary circumstances, and excluding the intervention of disturbing accidents and external forces and injuries, these means tend steadily to the accomplishment of the ends, and seem adequate to the purpose. Are the parts impressed at their original creation with properties which qualify them, under the circumstances specified, to accomplish these ends? Or is it necessary to suppose that they are either the same with the alleged vital principle, or are under the influence of this vital principle? The question, at this point, becomes metaphysical, and opens the path to discussions so complicated and extensive, that it is unseasonable to dwell on it longer.

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ART. III.—1. *A Course of Lectures on Dental Physiology and Surgery. Delivered at the Middlesex Hospital School of Medicine.* By JOHN TOMES, Surgeon-Dentist to the Middlesex Hospital. London, 1848. 8vo, pp. 397.

2. *Researches on the Developement, Structure, and Diseases of the Teeth.* By ALEXANDER NASMYTH, F. L. S., F. G. S., Member of the Royal College of Surgeons, London; Fellow of the Royal Medical and Chirurgical Society. London, 1849. 8vo, pp. 230.

3. *Observations on the Growth and Irregularities of Children's Teeth: Followed by Remarks and Advice on the Teeth in General. To which is added a short Essay on Artificial Teeth.* By W. H. MORTIMER, late Surgeon-Dentist to the British Embassy at Paris. Second Edition, Revised. London, 1845. 12mo, pp. 129.
4. *An Essay on the Teeth.* By AUGUSTUS COOK. London, 1848. 12mo. Pp. 75.
5. *On the Extraction of Teeth, with an Account of a Much Less Painful Mode of Operating.* By HENRY GILBERT, M. R. C. S. L., and Surgeon-Dentist, &c. London, 1849. 8vo. Pp. 66.

THE science of minute anatomy has made, by the aid of the microscope, so much progress during the last ten years, that the descriptions given in the best works previous to that time are no longer of the same value which they originally possessed. Of course different opinions must be entertained by different persons on the exact value of minute microscopical anatomy; and especially on the question, whether the knowledge of structure as revealed by the best observations tends essentially to improve the practical knowledge and art of the dentist. Nevertheless, as it is impossible to doubt that knowledge should in all cases be as accurate as possible, and as it is difficult to say in any given case what is to be the influence of speculative knowledge upon practical points, it cannot fail to be useful to understand the true structure of those parts, on which the dentist has to perform his delicate operations.

Mr Tomes is honourably distinguished as a microscopical observer of great diligence, skill, and accuracy; and the treatise which he has produced on the physiology and surgery of the teeth reflects great credit on him, both as an anatomist and as a dentist.

The work is in the form of lectures; and consists of sixteen discourses, which have been already before the profession in the pages of the Medical Gazette.

The first six lectures are devoted to the explanation of the anatomical structure and the functions of the teeth, including the history of the first and second dentition.

In the seventh chapter the author treats of abnormal conditions of the first dentition; in the eighth chapter, of abnormal conditions of the second dentition. In the ninth chapter are considered bony union of the teeth to each other, and to the *alveoli*; and fractures of the teeth and of the *alveoli*.

In the tenth chapter is given the history of caries ; in the eleventh that of nekrosis, exostosis, and abscess ; in the twelfth and thirteenth, various diseases affecting the pulp and periosteum, as irritation and inflammation. Next follow, in the fourteenth chapter, various diseases of the *alveoli*, as nekrosis and exostosis ; and diseases of the gums.

The fifteenth chapter is devoted to the subject of operations on the teeth, the reasons for these operations, rules to be observed in performing them, and the question of the induction of anæsthesia previous to extraction. And in the sixteenth and last chapter are considered the modes of attaching artificial teeth.

It will readily be understood that the anatomico-physiological part of the treatise is the most original. Readers will find in this part of the treatise, indeed, a most excellent history of the minute structure and formation of the teeth ; and one in which the author has illustrated the subject by the addition of several new facts.

The *Researches on the Physiology, Structure, and Diseases of the Teeth* by Mr Alexander Nasmyth, are on every account entitled to the greatest attention. The late Alexander Nasmyth was a man of great mental energy, ardent love of accurate knowledge, and indefatigable diligence as an anatomical inquirer. To these qualities were added mechanical ingenuity and dexterity by no means common, and very considerable practical skill as a microscopical observer. The fruits of the diligent exercise of these qualifications have appeared in various papers, the contents of which have been more or less fully brought before the readers of this Journal ; and though cut off in midtime of his days, it may be justly asserted, that he has earned a great and well-founded reputation as an original observer.

The present treatise, which has been published since his death by Mrs Nasmyth, may be regarded in the light of a bequest to the profession of the results of the labours of one, who has exerted himself in no ordinary degree in the cultivation of the scientific department of his profession.

The work is divided into eleven chapters. Of these, the first contains several observations on the physiology of the teeth in different classes of animals. In the second is given the descriptive anatomy of the Mouth and Jaws ; and in the third the descriptive anatomy of the Teeth. The fourth chapter is devoted to the general and minute anatomy of the teeth ; the fifth to the development of the formative organs of the Teeth ; and the sixth to the general and minute anatomy of the dental capsule and pulp. In the seventh chapter the author gives the history of the development of the permanent Teeth ; in the eighth he considers the characters of the Teeth as an indication of age ; and the ninth

chapter furnishes an ingenious disquisition on the Teeth as an indication of the progressive improvement of the human race. The tenth chapter contains the history of the development of Ivory ; and the eleventh is on the chemical composition of the Teeth.

The descriptions are illustrated by numerous beautiful woodcuts and a series of ten excellent engravings.

No part of the volume is expressly devoted to the explanation of the diseases of the teeth ; but in various parts the knowledge of the structure is incidentally applied to elucidate the origin and nature of those diseases to which the teeth are liable.

We entered so fully into the subject of both these works when considering the Odontography of Mr Owen in the fifty-fifth volume and the three memoirs of Mr Nasmyth in the fifty-seventh volume of this Journal, that it is unnecessary at present to enter into formal analytical accounts of either. It may be proper, nevertheless, to advert to certain points in which both authors communicate instructive information.

It is known that the teeth are not homogeneous bodies, and that they consist of several different substances or elementary tissues. These Mr Nasmyth distinguishes into five ; 1st, Cortical Substance, also denominated *Crusta petrosa*, or *Cementum* ; 2d, Enamel, or adamantine substance ; 3d, Ivory ; 4th, Corpuscular Ivory ; and 5th, Canalicular Ivory. As, however, it is allowed that the two last substances are in truth modifications of the third, it results that the teeth consist of three elements ; namely, cement or cortical substance, enamel, and ivory.

Of the five substances enumerated by Mr Nasmyth, three, namely, those now specified, are constantly to be found in the teeth of man and the higher MAMMALIA. The fourth, that is corpuscular ivory, is met with only occasionally ; while the fifth, canalicular ivory, is confined in its distribution to particular animals.

The dental elements are divided by Mr Tomes into three ;—namely, Dentine, or tooth substance ; 2d, Enamel ; and, 3d, Cement, or, more properly, toothbone. The dentine, or tooth substance of Mr Tomes, corresponds to the ivory of Mr Nasmyth and other writers ; and Mr Nasmyth disapproves of this innovation, and pronounces the whole of these new names as objectionable, and dentine as the most objectionable of all, for the reason that it forms not only an uncalled-for addition to scientific nomenclature, already too multiplied, but that it conveys, by its terminal syllable, the idea of the substance so designated being an elementary principle. This, however, is not the case ; and Mr Nasmyth maintains that enamel and cortical substance are as much entitled to the designation of Dentine as ivory.

This explanation is necessary in the outset, in order that the reader may have a correct notion of the substance to which the descriptions now to be given apply.



When a tooth is divided by section, or fractured by the blow of a hammer, the following relative position of parts is recognised :—*First*, In the centre, are seen one, or two, or three cavities, longitudinal, and containing a soft vascular substance. This substance is called the pulp, and the cavity is called the pulp cavity. *Secondly*, Immediately exterior to this is placed a substance of some thickness, and which is the ivory of Mr Nasmyth and the dentine of Mr Tomes. *Thirdly*, Exterior to this substance, and at the superior part of the tooth, or what is named the crown, is placed a thin covering of very hard, pearly-white matter, which, however, is not continued lower down than the neck of the tooth. This is the enamel. It is not uniformly of the same thickness. It covers the most prominent parts of the crown of the tooth, and is sometimes thicker at certain parts than at others. In the *fourth* place, over the root of the tooth, whether simple or compound, is extended, from the neck to the root, a grayish-white substance, void of the pearly aspect of the enamel, and which covers, like a capsule, not only the root, but sometimes, it is said, in a thin layer the enamel. This is the cortical substance. In the teeth of those animals which are simple, this is found on the exterior only ; but in the elephant and other animals, which have what are called compound teeth, that is, several teeth united in one piece, it is found uniting and holding together these component simple teeth, and, on this account, it has received the name of Cement.

Ivory is darker in colour than enamel, and has more or less of a yellow tint. Its exterior is always covered by an investment of enamel, or of cortical substance. Being softer than enamel, it yields more quickly to the action of attrition ; but as it is very tough, it is better adapted for forming the body of the tooth. The toughness, yet softness and sectility, of ivory, have been made available for the necessities and luxuries of man from the earliest to the present times, among the rudest inhabitants of the globe as among the most refined ; and ivory has been most extensively employed in the most elegant manufactures. The workman is aware that ivory possesses a kind of grain running in the vertical direction of the tooth. This grain he compares to the course of the fibres in wood, and he knows that a carving made in the direction of the grain will be smooth, while in the opposite it shall be rough. Ivory is liable to fall into a state of decomposition, or rather disintegration, in which it splits into concentric plates corresponding with the vertical direction of the tooth. Of this change a common example is presented in the fossil teeth of the mammoth or primigenial elephant ; and occasionally in human teeth in a state of decay. A transverse section of ivory exhibits a symmetrical arrangement of concentric circles, crossing each other in every direction. Acids dissolve ivory, though less quickly than enamel ;

and it becomes softened and destroyed in what is termed natural decay, as well in the deciduous as in the permanent teeth. The disintegration of ivory is also evinced in the removal of the fangs of the deciduous teeth, and sometimes of the permanent teeth by absorption.

Of the Dentine or Ivory the first and most important of these substances, as constituting the large mass of each tooth, Mr Tomes gives the following account :—

“ Dentine is made up of two distinct parts, *dentinal tubes* and a uniting medium—an *intertubular tissue*. The tubes have distinct parietes, which nearly or quite equal in thickness their calibre, and although usually empty, yet sometimes even in healthy dentine, appear to contain a minutely granulated substance. Their arrangement is radiate, the centre of radiation being the pulp-cavity, or a vascular canal. Each tube commences on and contributes its share to form the wall of the pulp-cavity, or of a vascular canal, from which point it advances in an *undulating* course towards the surface of the tooth; the general direction being nearly rectangular with the surface from which it started. If a single tube be traced through its whole length, it will be found to have made two or three large bends, and in addition to these, which are called the primary curves, a vast number of small undulations; these latter are termed the secondary curves.

“ In their course, the dentinal tubes give off branches, and these meeting with others of similar character, anastomose, and thus form frequent connections, throughout the whole substance of the tooth. The nearer the tubes approach the enamel, or the cementum, the more frequent are their branchings, till at last they terminate either by anastomoses with adjoining tubes, or pass into the external structures,—or else terminate in a dilatation,—or become so extremely minute, that they are lost to the sight.

“ Such are the general characters of the dentinal tubes. They are subject, however, to many modifications. Those which pass to the enamel differ slightly, in their comportment, from those which go to the cementum. Then, again, those situated near the end of the fang vary from those near the neck of the tooth.

“ These modifications we must now consider.

“ The dentinal tubuli of the crown of the tooth are best seen in a vertical section through the pulp-cavity, reduced sufficiently thin to be examined by transmitted light. In this situation, the tubes at their commencement are so closely packed, as to leave but little room for intertubular tissue; as they advance, however, in their divergent course towards the surface, and become further separated from each other, the individual tubes may be traced to their terminations, and the intertubular tissue recognised.

“ In well-developed dentine, the tubes in the crown of the tooth are distributed at tolerably regular intervals. Those, however,

which are destined to pass towards the masticating surface are rather more closely placed at their commencement, than are those which go towards the sides of the tooth. Their divergence is also pretty uniform throughout their whole course; and the primary and secondary curves are well marked. Very few of the coronal tubuli give off branches, till they have arrived close to the enamel: then they divide into two branches, and one or both of these may subdivide, and then terminate; but the subdivision is not constant. The absence of branches from the dentinal tubes in the crown of the tooth constitutes their greatest peculiarity, and also distinguishes the human tooth from most others. In the teeth of many of our domestic animals, the coronal tubuli give off innumerable branches. In the human tooth, too, a few tubes may be found that give off single branches about midway between the pulp-cavity and the enamel. These coronal branches very commonly recurve, and anastomose with similar branches from neighbouring tubes.

"Cells are but seldom found in the coronal dentine. They are, however, occasionally met with, and when present are usually large and elongated, and placed at an angle with the tubuli, which latter run into them. The occurrence of cells in this part of the tooth is in well-formed teeth unusual, while, in imperfectly-developed teeth, they are very numerous and irregular in size and shape; hence their presence in any case must be regarded as an indication of imperfect development.

"The coronal tubuli suffer but little diminution in size, till they divide, and then the branches conjointly are larger than the parent tube. Sometimes, indeed, two tubes unite near the enamel, and, from the larger tube so formed, two or three minute ones are given off.

"In some specimens, however, the tubes, during the greater part of their course, are subject to slight dilatations. These occurring at short and tolerably regular intervals, and in a number of neighbouring tubes, at similar points in their course, give a striated appearance to a moderately thick section, if viewed by transmitted light. The striæ are of course transverse to the direction of the tubes, and are short, extending, perhaps, across twenty or thirty tubes, and then stop short.

"The terminal branches, having arrived at the line of junction between the dentine and enamel, are there lost, or recurve and anastomose with contiguous tubes, or pass across the line of junction into the enamel, or else end in a dilated extremity near the surface of the dentine.

"The coronal surface of the dentine, previous to the development of enamel, is marked by numerous shallow hexagonal depressions, while, on the corresponding surface of the pulp-cavity, the open ends of innumerable tubes are found.

"In the tubuli which occupy the neck of the tooth, we find the first modification of character from those of the crown, and this increases in degree the farther we recede from the crown, and advance towards the end of the fang. Instead of pursuing an uninterrupted

course, till within a very short distance of the termination of the dentine, and then dividing into two secondary branches, like those of the crown, the tubes of the neck of the tooth give off, during the latter third of their course, numerous fine hair-like branches, visible only under a very high magnifying power. The parent tube, at the same time, gradually decreases in size till it is no longer traceable. A few others, on the contrary, divide in the horizontal plane into two principal branches, and from these the more delicate ones proceed. Then, again, some dilate at their extremities, and from the dilatation give off minute branches.

"In the fang, and especially towards the apex, the dentinal tubes divide, subdivide, and again and again, during the whole or greater part of their course. Many of these, from their numerous branches, and the small angle at which they leave the parent tube, resemble a leafless branch of willow. Sometimes the branches are given off from one side only of the parent tube; more commonly, however, they proceed from both sides. In other cases, again, the primary tube gives off alternate minute branches from each of the secondary curves, and from these again even lesser branches start.

"The tubes situated in the middle part of the fang, usually begin to throw off branches in about the middle of their course; those situated near the neck of the tooth commence dividing rather farther from the pulp-cavity, while others near the apex of the fang give out branches almost from their commencement. It is by no means uncommon, however, for a primary tube, previous to any division, to dilate, receive branches from other primary tubes into the dilatation, again contract, and resume its original course. The division into secondary branches, as stated by Mr Bowman, occurs mostly in the horizontal plane.

"In the fangs of teeth, the anastomosis between the tubes is far more frequent and general than in the crown. The occurrence of radiating cells (similar to the cemental cells) amongst the tubes, and especially towards the outer surface of the dentine, is not uncommon. The terminal branches of the dentinal tubes, that occupy the neck and fang of the tooth, are either lost in the intertubular tissue, or they may be traced into radiating cells situated amongst the tubes, or terminate by anastomosing with adjoining tubes, or else pass into the interspaces that exist between the large granules that form the outer surface of the dentine of the fang, or they pass across the line of junction between the dentine and cementum, and communicate with the cemental cells.

"The primary and secondary curves are well marked in the tubes, situated in the neck and in the middle portion of the fang. but at, and near, the extremity they become very irregular, or cease to exist. Those tubes that occupy the dentine situated between the fangs and the pulp-cavity, of molar teeth, commonly follow a spiral course.

"In the fang the distribution of the tubes is much less regular than in the crown of a tooth. They are frequently found arranged in small groups, with intervals in which they are less numerous.

They are generally much less crowded, too, at their commencement in the pulp-cavity in the fang, than in the crown.

"I have told you that the point of greatest diameter of the dental tubes is at their commencement on the walls of the pulp-cavity, though, in their course previous to the division of the trunk into branches, they suffer but little loss in calibre.

"When measured by the micrometer, the internal diameter of the largest tube is about the  $\frac{1}{10000}$  of an inch, and the diameter including the parietes,  $\frac{1}{8000}$  of an inch. The dental tubuli, when examined in their length with a low magnifying power, and by transmitted light,—that is, when the object is placed between the light and the eye,—appear as dark lines; but, if a high power be used, then they are marked by two dark lines, with a narrow line of bright light interposed. In some few instances, the light appears to be prevented from passing through the central line of the tube, by the presence of granular matter in its interior, but it is an exception to the usual condition.

"In many animals, especially in fish, the tubes are so large, that this appearance of a light and two dark boundary lines is seen even with a low magnifying power. The teeth of the wolf-fish afford a good example. If, in human dentine, a transverse section of the tubes, at their commencement, be viewed with a high power, the ends of the tubes will be seen; the area and the parietes, with the surrounding interbular tissue.

"The dark line so constantly seen with a low power is, I think, no proof that solid matter is contained in the tube; for the appearance described may be exactly imitated by introducing minute globules of air into a fluid, and then placing it on the field of the microscope, when, by transmitted light, each air globule will be bordered by a dark line, while the centre is light. If the globule of air be very small, it will appear altogether dark. The dark line, in this case, is dependent for its origin upon the altered course of the rays of light, which is effected by the curvilinear form of the globule refracting the light at such an angle, that it precludes the possibility of the rays passing through the lenses of the instrument. Hence arises the darkness of the object. I conceive that the dental tubuli, when occupied by air, refract the light in a similar manner, and thus appear dark.

"If the dental tubes be examined by reflected light,—that is, if the section be placed upon a dark ground, and the light be made to fall upon it instead of passing through, as in the former experiment,—then the tubes will be seen as opaque white lines placed in a transparent medium. This appearance would at first sight seem to indicate, that the tubes are filled with solid granular contents; however, in this as in the former instance, the appearance can be imitated without the presence of granular matter. If a transparent substance, as glass, be taken and reduced to powder, and the powdered mass be examined by reflected light, it will appear as an opaque white substance, studded with transparent particles. In this instance the opacity is clearly due to the reflection from the minute particles being at such angles that the rays of light pass external

to the field of the glass. The dentinal tubes have, probably, a similar effect on the light."—Pp. 32–41.

"In the temporary, and not unfrequently in the permanent teeth, the tubes, instead of presenting an uninterrupted line, present on their surface numerous indentations, just as though they were composed of a series of hollow beads, which were united and made to communicate with each other by small apertures. This appearance has led Mr Nasmyth to conclude that the teeth, instead of being composed of tubes, are made up of beccated fibres; of these he gives a drawing, separated from the intertubular tissue and from each other. This appearance, however, admits of another explanation, which will be given after the development of the dentine has been explained."—P. 42.

The second part, composing the ivory or dentine, is what Mr Tomes calls, according to his hypothesis, the Intertubular Tissue. This substance, as its name implies, occupies the space between the tubules, everywhere surrounding and investing them, and thereby contributing to render the whole ivory a solid dense mass, the interior of the tubes and cells alone being hollow.

"It would be difficult to estimate, beyond mere guess," says Mr Tomes, "the relative amount of this structure in a tooth, as distinguished from the parietes of the tubes, since the quantity varies somewhat in different parts of a tooth. Thus, in the coronal dentine, near the pulp-cavity, the tube-tissue preponderates; while in the dentine, near the surface of the fang, the intertubular tissue forms the larger element.

"In a section either of the crown or of the fang, made near the pulp-cavity, so as to cut transversely the dentinal tubes, before they have suffered any diminution in size, the intertubular tissue may be plainly distinguished from the parietes of the tubes. If, however, the section be extremely thin, the recognition will be difficult; or, if acid be added to the section, then the distinction will be lost, though before the addition it was ever so plain.

"The intertubular tissue is itself made up of minute granules, closely united.

"In the dentine situated near the pulp-cavity, a granular appearance can alone be recognised, and even this can scarcely be seen in some specimens. Near the periphery of the dentine, however, the granularity is more marked, and the individual granules may be seen and even measured. They are spherical, somewhat irregular in size, varying in different specimens, and in the same specimen, from the  $\frac{1}{100000}$ th to the  $\frac{1}{100000}$ th of an inch in diameter. The granules are united to each other by the interposition of a second substance, which is recognisable only at those points where from their spherical form the granules do not touch each other.

"At and near the surface of the fang, when the tubes have become very small in size, the uniting medium is often partially absent; hence we have intergranular spaces, of such form and size as would occur between partially united spherical granules. In addition to these, we have a few oval branching interspaces.

"These intergranular spaces give to this part of the tooth a general appearance of granularity; so that, in viewing a section through the pulp-cavity, in the length of the tooth, on looking to the fang, we see at first sight the dentinal tubes. These diminish in size, are succeeded by a layer of granular-looking tissue, and lastly comes the cementum, which forms the surface of the fang."—Pp. 46–48.

This is the proper place to mention, that a controversy has arisen among the microscopical authorities as to the exact nature of the structure of the ivory; one party, at the head of which may be placed Leewenhoeck among former observers, and among late observers Purkinje, Retzius, and Mr Tomes, maintaining, that the elementary fibres consist of hollow tubules placed side by side, and united by an intermediate substance; while the other party, of which Mr Nasmyth may be considered as the representative, consider the component elementary parts to be strings or filaments of bead-like bodies.

Ivory has been regarded, says Mr Nasmyth, as a structure composed of minute branching tubes, which radiate from the surface of the pulp to the periphery of the tooth, where they terminate; in some instances by communicating with cells resembling the corpuscles of Purkinje. The central extremities of these supposed tubes originate by open mouths on the walls of the cavity of the pulp; and their canals, though more or less filled with calcareous matter, perform the function of affording passage to certain fluids. The advocates of this doctrine further represent the material which connects the tubes to be amorphous and structureless.

In opposition to this view, Mr Nasmyth finds, after very careful microscopical observation, aided by the results of mechanical means and chemical experiments, that, after solution in hydrochloric acid, the animal residue consists of solid fibres, which present a peculiar baccated or beaded appearance; in short, that the filament represented as a tubule, is in reality a solid fibre, composed of a row of minute bodies arranged in the linear direction like beads strung on a thread.

Mr Nasmyth further represents the intertubular, or what he denominates the interfibrous material of ivory, to be not distinct from the tubules, but to form a part of them; that the fibres and the interfibrous material have a compartmental or cellular arrangement; that the fibres are rows of persistent *nuclei* belonging to the cells of the interfibrous material; and that these elements are associated in such a manner as to constitute one tissue, in which all appearance of separate parts is generally lost. When the cellular appearance is met with, it is accidental and morbid, indicating at least the tendency to decay. To illustrate the arrangement now mentioned, Mr Nasmyth gives, at pages 90, 93,

and 94 a series of twelve figures, showing the bead-like arrangement and the cellular appearance in different stages.

Such are the opposite views. It is not easy to say who is in the right. The question depends so thoroughly upon microscopical inspection, and the manner of explaining the results of this inspection, that though this may throw some light upon the subject, yet it seems altogether inadequate to determine it in such terms, as to obviate objection and be free from the imputation of error.

Mr Nasmyth maintains further, that the cellular arrangement which he describes in the ivory is altogether different from that referred to by Retzius.

Assuming that this representation of the atomical constitution of the ivory is correct, Mr Nasmyth infers, that the arrangement which he describes is most perfectly adapted for strength and durability. Thus the highly organized tissue of the ivory is protected from the operation of external agents by the impermeable covering of the enamel; while the ivory itself is so constructed as to present a more dense conformation at the periphery than at the central part of the tooth. At the periphery the fibres are minute, and interlace with each other; near the centre they are long, and arranged in a parallel direction. The interfibrous substance, upon which he maintains the great strength of the ivory depends, is accordingly more abundant at the surface than near the centre. Again, the interfibrous substance is not solid throughout, but composed of small masses packed together in cells, and constituting a series of arches, from the neighbourhood of the pulp to the periphery of the tooth, these small masses being accurately adjusted to each other, and having an imbricated arrangement.

These cells, Mr Nasmyth states, present a perfect analogy with cells in other situations, differing from the cells of epithelium only in the nature of their contents, and in the lateral position of their *nucleus*. The nuclei further are sometimes double, as occasionally occurs in the epithelium.

The concentric arrangement of these cells, Mr Nasmyth further represents to be the cause of the laminated decomposition (disintegration) which takes place in teeth.

The ivory, as now described, forms what Mr Nasmyth denominates *fibro-cellular* ivory. It is the most perfect form of that substance, and is most extensively distributed in the animal kingdom. In its most characteristic form it is presented in the MAMMALIA and REPTILES; and forms the largest proportion of the teeth of man.

The second variety of ivory, or that which Mr Nasmyth denominates *canalicular*, is traversed in a vertical direction by numerous canals. This is found in the teeth of FISHES and a few other animals, as the *Orycteropus*.



The third variety, or that which Mr Nasmyth denominates *corpuscular* ivory, he represents to result from the conversion into ivory of that portion of the pulp which remains after the completion of the fibro-cellular sort, and consequently to be in immediate connection with the latter. Examples of this species of ivory are found in the teeth of the walrus, sloth, and some other animals. In some instances it is observed to be the product of morbid action. It appears, in short, to be formed by the ossification, partial or general, of the pulp. This is represented to be of frequent occurrence in the human subject; and Mr Nasmyth states, that in his practice, it was generally the sequela of long-continued disease, either of the tooth itself, or of some part of the mouth. He allows, however, that there is no evidence to prove that it may not take place without previous morbid symptoms.

Sometimes in the tusk of the elephant, this variety of ivory is seen occupying a considerable extent of the pulp cavity, and presenting singular modifications of configuration; in one instance resembling beads variously arranged; in other instances having the shape of stalactitic projections. As this substance possesses *corpuscula* similar to those of bone, Mr Nasmyth applies to it the name of *corpuscular* ivory.

The Cortical Substance or Cement is always found in the peripheral part of the tooth, forming an investing layer, and lying in close apposition with the enamel in the ivory. In the elephant and other *PACHYDERMATA* it constitutes a considerable and important portion of the molar teeth; and the same in the Ruminant Tribe.

The cortical substance usually occupies only the exterior of the teeth. But as in some animals, for instance, the elephant, it surrounds the simple or integral teeth, and holds them together, it appears to enter into the formation of their substance. When the cortical substance in conjunction with the enamel and ivory of one of the convoluted teeth of the pachydermatous animals, is subjected to the attrition consequent on the mastication of vegetable food, it is worn away more speedily than the other dental substances, by reason of the greater softness of its texture. In like manner the ivory yields to attrition sooner than the enamel; and the result of this is, that the three substances present a series of alternating ridges of different degrees of elevation.

The cortical substance presents, according to Mr Nasmyth, in intimate structure, the characteristic corpuscles and canals of bone, the latter being filled with osseous matter, but otherwise resembling the canals of Havers.

According to Mr Tomes, tooth bone or cement is similar to bony structure, being composed of minute granules about  $\frac{1}{1000}$  of an inch in diameter, closely united. Dispersed through this granular tissue are cells from which proceed numerous tortuous tubes, which freely anastomose with each other, and with those

arising from neighbouring cells. By this arrangement, a net-work of cells and tubes, permeable by fluids, is carried throughout the whole mass.

The objects termed *corpuscula* by Mr Nasmyth are manifestly the cells of Mr Tomes.

The cement is gray in colour and porous, and, in consequence of this porosity, it acquires in herbivorous animals a dark colour, and in children fed on vegetable food it generally receives a brownish or greenish tinge. This property which cortical substance possesses of receiving and retaining within its porous interstices colouring matter of various kinds, affords a means by which it may be distinguished from enamel and ivory. The appearance in the teeth of horses, denominated mark of mouth, and by which the age of the animal may be conjectured, is owing to the presence of cortical substance. It appears, indeed, to be, in all respects, a species of bony matter.

It is to be observed, nevertheless, that cortical substance has no organic connection with the enamel or ivory, close to which it lies. Mr Nasmyth, accordingly, is opposed to the idea which has been supported by Mr Owen, that "the chief use of the cortical substance is to form the bond of vital union between the dense and commonly unvascular constituents of the tooth and the bone in which the tooth is implanted;" and maintains that no vital union subsists between it and these textures, and that its union is mere mechanical apposition, and its purpose in like manner mechanical.

To this view, on the other hand, Mr Tomes seems opposed. The cement, when present in any quantity, he represents to be traversed by canals and blood-vessels.

The cement, as it encases the dentine, follows the curved surface of the fangs, often uniting in one conical mass two or even three fangs, exactly as it agglutinates, many small or integral teeth into one large tooth. In this manner, the roots of the second and third lower molar teeth, and those of the *dentes sapientiæ* of the upper jaw, are not unfrequently connected. When such union exists, the amount of cement is double, as the two cortical layers of the two fangs are united by what would otherwise have been the external surface of the folds of the cement. In such teeth also, the cement is commonly pierced by one or more canals for blood-vessels.

Upon the necks of the teeth the cement appears in a very thin layer, which is here traversed only by minute tubes, which, commencing on the surface, pass horizontally inwards towards the dentine. Lower down the root the layer becomes thickened; and it is in this cement that are observed the cells with branching tubes, already mentioned. If the layer of cement become still thicker, it is provided with canals for blood-vessels.

In arrangement cortical substance presents the appearance of concentric laminæ, the centre of the tooth being their common centre.

These cells are not uniform in size. They vary from the  $\frac{1}{10000}$ th part to the  $\frac{1}{1000}$ th part of one inch. Their form is oval, sometimes round, and occasionally fusiform. The inner walls are nodulated. To this irregularity of surface Mr Tomes ascribes the dark aspect of the cells when examined by transmitted light. The outer surface of the cortical substance is nodulated. The granules project, and form such a surface as might be presented by half-imbedded fig seeds.

In the teeth of the lower animals, especially those of the *edentata*, in which enamel is wanting, the cement forms a large proportion of the tooth. It is, when exposed, highly sensible to touch, unless when, from any cause, it has lost its vitality.

Cortical substance or cement is extended in a very attenuated or rudimental form over the enamel.

The enamel is the hardest, the least constant, and the most sparingly distributed of all the dental elements, since, in some animals, it merely supplies the teeth with a hard edge to divide the food. In man, it covers the crown of the tooth and ceases at the neck, being about half a line in thickness on the grinding surface of the crown, and becoming thinner on the sides. On the teeth of the hippopotamus and elephant, it varies from half a line to one-fourth of one inch in thickness.

Enamel is the hardest and most solid of all the animal textures, and contains the largest proportion of calcareous matter. It is so dense, as to be capable of receiving a very fine polish. It turns the edge of cutting instruments, and elicits sparks when struck with steel. From the latter circumstance, it might be inferred that it contains a little silica; but this has not been ascertained to be the case. The enamel of the incisors yielded to chemical analysis 3·59 of organic, and 96·41 of inorganic matter. The relative proportions of these two orders may be inferred from the following results of the analysis of the molar teeth of an adult:—

Phosphate of lime, with a trace of fluuate of lime,	89·82
Carbonate of lime,	4·37
Phosphate of magnesia,	1·34
Salts,	0·88
Chondrine,	3·39
Fat,	0·20

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100·

Notwithstanding this great hardness, it is brittle; and when its adhesion to the ivory is in any manner disturbed, it is easily broken, and may be chopped off with little force. The chief mode of its destruction is in consequence of previous decay of

the ivory, when it either moulders away, or is cracked and detached. When exposed to the influence of heat, it cracks from the ivory, and is readily acted on by acids. On the other hand, if the enamel happen to be broken by external violence, and the fluids of the mouth act on the ivory, the latter speedily decays and is disintegrated.

Enamel is composed, according to Mr Nasmyth, of cells arranged in regular rows, forming composite fibres placed at nearly right angles to the surface of the ivory, the original *nuclei* of the cells not being persistent. Mr Tomes represents it to be composed of dense semitransparent fibres, placed side by side, and closely united. These fibres, as we understand Mr Tomes, form a sort of net-work of six-sided prisms, tolerably uniform in size, being from  $\frac{1}{10000}$ th part to  $\frac{3}{10000}$ th part of one inch in diameter. The transverse section exhibits the hexagonal arrangement of these fibres, but the longitudinal section presents them as irregular squares.

The direction taken by the enamel fibre Mr Tomes represents to be, for the most part, vertical to the surface of the dentine on which it rests. Those fibres, therefore, which proceed from the flat surface of the crown rise vertically, while those issuing from the lateral surface of the tooth are horizontal. When the coronal surface of the ivory is concave, the enamel fibres of the opposite sides of the concavity form with each other angles, and meet at their external ends, or are bent out of their course. This junction is frequently imperfect, leaving a fissure, under which the ivory, being less protected from external influence than on the other parts of the crown of the tooth, is more frequently attacked by disease.

The enamel fibre, generally solid, has sometimes, nevertheless, an extremely minute cavity running through the whole or part of its length. This cavity is best seen in newly-developed enamel; but a trace of the canal may sometimes be seen in the enamel of adult teeth. Interposed between these enamel fibres are the remains of the membrane in which they have been developed, and which, when hardened by the reception of earthy matter, serves to connect these fibres. This tissue, however, cannot be traced, except in imperfectly-developed enamel, and by the aid of acids.

The enamel fibres, Mr Tomes infers appear to be developed in sheaths of membrane, and united to each other by the adhesion of the sheaths and subsequent calcification of the membrane in common with the enamel itself. From the results of chemical analysis, it seems reasonable to believe that this membrane consists either of gelatine or chondrine.

The union of ivory, cement, and enamel, as now described, forms the tooth. The materials by which each is distinguished, are the product of blood-vessels proceeding from the pulp, and perhaps

the alveolar periosteum. These vessels, however, are believed to run only in the interstices between the fibres of each tissue, and not in the substance or fibres of the tissue itself. These vessels are numerous in the early and growing stage of the tooth elements. But after these have been fully formed and deposited, the vessels become greatly diminished in number and size, until, in certain of the tissues, they may be said to cease to exist. In this respect, however, different degrees of organisation may be distinguished.

The cement or cortical substance, when collected in any amount, is provided with vessels as well as with cells and radiating tubes in connection with the vascular surfaces. The cement, therefore, is the most organised of the dental elements. In these respects, indeed, it approaches to common bone. Ivory, in which are sometimes, not constantly, found vessels, has in all instances its tubes or capillary pores opening directly on a vascular surface,—the pulp. This, therefore, though little organized exteriorly, yet presenting traces of vascular apparatus at its internal or central aspect, may be placed next to the cement in the degree of organisation character. The enamel, on the other hand, which is void of vessels, and is connected with these merely by the ivory and its tubules, must be regarded as unorganised, yet as a product of organisation.

Ivory seems to be a modification of cement. Mr Tomes regards the ivory tubules as elongated cement cells.

On the relative value of different sorts of ivory, in a commercial point of view, and in the arts, Mr Nasmyth has given some interesting information, which, he states, is derived from Mr Russell, one of the most intelligent ivory merchants in London :—

“The first in density is Ceylon, and is derived from the countries around Ceylon. This ivory resembles Siam in quality, but does not run so regular as the latter. In size, the Ceylon teeth are much smaller than those of Bombay, seldom exceeding 70 or 80 lbs., and averaging generally 30 or 40, while the Bombay sometimes run to 180 lbs., but this is an extreme weight. Ceylon teeth have more of what is called green in them than those of Bombay, and resemble African teeth in that respect. The price much the same ; the difference betwixt them not being generally known amongst buyers. There is, however, a difference of value amounting to 6d. per lb, (from the smaller quantity of green) in the Ceylon teeth. It is generally used for the same purposes as Siam.

“African ivory is found to hold a place next to that of Ceylon in specific gravity, although in commerce it has the credit of being the heaviest. It is possessed of the cleanest grain, and sells at the highest price. It is brought from the Gold Coast, and is principally used for cutlery, fancy turnery, combs, &c. There are three different appreciable qualities derived from the different portions of each tooth. The first or heaviest kind of white ivory is that obtained from the point of the tooth ; what is called green, and has a pel-

lucid appearance, is derived from the centre of the tooth, and is the most valuable portion ; and the coarse white is from the outside of the same portion ; and, although heavier, not so much esteemed.

“ The ivory next in density is that from Siam. It is next in estimation for quality and texture, and obtains nearly the same price in the market as African ivory. It is principally used for the same purposes as the African, but is not so valuable, from its supposed inferiority in weight, which the specific gravity shows to be real. It is principally green, but does not keep its colour so well as African green, changing to all shades, sometimes to a pinky hue. Siam teeth are more diseased than any of the others that are brought to market ; yet the diseases attacking these teeth do not run so extensively through the whole texture of the tooth, and thus do not deteriorate the tooth so much for use.

“ The ivory next in density is that of Bombay. It displays two qualities in its structure, which are applied to different purposes. The fine quality is from the internal part of the tooth, but is of less specific gravity than what is denominated coarse, and derived from the exterior. The rationale of this fact is evident from the structure of ivory. The peripheral portion of ivory has finer fibres than the interior. The fibre being the animal or light portion, necessarily betrays a less specific gravity than where the substance is more exclusively earthy. The peripheral cells of the ivory are also larger than the internal, which accounts for what is called the coarseness of grain in that part. It is observed that the specific gravity of the point of the tooth in African ivory is great, and also that of the peripheral portions, demonstrating the presence of a greater proportion of hard concrete matter in those parts which are exposed to the contact of hard substances.

“ The Bombay teeth are used for cutlery, turnery, and combs, but principally for pianoforte keys ; no other kind is so well adapted for that purpose, it being so white and opaque, or *dead*, as it is called. It, however, very soon gets yellow, and it has no tint of green. There are larger and heavier teeth derived from this market than from any other, running sometimes to the weight of 180 lbs. per tooth. The Bombay ivory is softer than the kinds previously noticed. The quantity annually consumed in London for pianoforte keys is about twenty tons. Its price is much the same as that of the other kinds.

“ The ivory from the Cape Coast having a specific gravity of 1·742, is invariably white and coarse. It is held in commerce to be the lightest kind, and has the character of being marked by a number of yellow concentric rings. Its price is low, and although coming from Africa, is not recognized as African ivory.”—Pp. 211–213.

Both authors give minute accounts of the development of the teeth, and Mr Nasmyth gives a description of that of ivory, which is very interesting, and has every appearance of being correct. Upon this part of the subject, however, we cannot enter ; and it is the less requisite, as the subject of development of the teeth is accurately explained in the paper of Mr John Goodsir, published

in the fifty-first volume of this Journal, and as both authors follow closely the tract of that anatomist.

Decay of teeth, or at least of ivory, Mr Nasmyth maintains, on the testimony of chemical analysis, to consist in the withdrawal of earthy matter. He allows, however, that it is a process at once of vital and chemical action, and more particularly of the former. This is only in accordance with what is known, so far as the subject is understood, to take place in other morbid changes. The vital influence appears first to be impaired or in some manner deranged ; and then chemical actions operate more energetically or less so, but in such a manner as to change the structure of the part. Decay commences in the peripheral portion of the tooth at the greatest distance from the pulp, the vascular and most vital texture. Mr Nasmyth infers, that whatever change is produced, must be carried on through the agency of the substance interposed between the diseased part and the source of vitality, the pulp.

Mr Nasmyth has often observed occupying the pulp cavity of the tooth of the elephant, that of the walrus, and occasionally the cavity of the temporary tooth of the tiger, a peculiar looking substance, almost black in colour, but in truth of a deep ruby-red, and of brittle consistence. It occurs in masses as large as nuts ; and one of these, about three inches long, three quarters broad, and about half an inch thick, he presented to Dr Robert Dundas Thomson. This piece from the tooth of the elephant was attached to the wall of the cavity by one end. The tooth which contains this dark ruby-like substance is generally very irregular in structure, and has the marks of diseased action and faulty development, together with the appearance of sinuses running through the body of the ivory. The dark portion of the material displays, when examined by the microscope, no structure ; but the portion attached to, or in immediate neighbourhood of it, displays, under a quarter of an inch object glass, a series of shrivelled cells, of the same appearance as the irregular ivory in immediate connection with it. From its position and connection, Mr Nasmyth thinks that this ruby-coloured matter is a portion of the dried pulp, which was in a modified state from disease. From the colour of its coarse powder, this substance has been named *Pyropin*.

Dr Thomson found that sections of it exhibit partial traces of organization. The substance is insoluble in water, and thus differs from glue or gelatine, to which, in physical aspect, it bears some resemblance. Two analyses gave the following results as to chemical constitution :—

	First.	Second.
Carbon,	53.33	53.50
Hydrogen,	7.52	7.66

	First.	Second.
Nitrogen,	14·50	} 38·84
Oxygen, }	24·65	
Sulphur, }		

When pyropin is boiled in water, the liquid is not precipitated by infusion of galls, proving that it contains no gelatine ; neither is it precipitated by acetate of lead. When broken into coarse powder, it has a rich ruby-red ; in fine powder, it is brown. A small portion, dissolved in hot alcohol, was deposited on cooling, in the form of ferruginous flocks. Whether it be derived from the blood, or from the pulp, is a question still open to inquiry.

The limits within which this article must be confined, prevent us from dwelling longer on these works. The account already given, however, will, we trust, communicate an idea of the merits of both performances, and induce readers to study them attentively. Both are most valuable in the accurate information which they communicate on the structure of the teeth. The treatise of Mr Tomes is very ample on diseases, treatment, and operations, and must be a most serviceable guide to the practical dentist.

The small treatise of Mr Mortimer will be serviceable as a popular guide to parents and others, who ought to have some knowledge of the management of dentition in children. The instructions are judicious ; and Mr Mortimer shows that he is well acquainted with physiology, and does not lose sight of common sense.

The performance of Mr Cook is still more compendious, and is, on that account, not without its use to general readers, to whom his directions on the management of the teeth are principally addressed.

The object of Mr Gilbert is to make known a more efficient and less painful method of extracting teeth than that in common use. This is accomplished by placing the individual in a chair of a peculiar construction, and so fixing the head and jaw, that the tooth is extracted in the direction of the axis. This is probably an advantage both to the operator and to the patient ; and Mr Gilbert promises to obviate the accident of fracture of the jaw or alveolar processes, which he represents to be not uncommon, according to the present method of procedure among dentists.

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ART. IV.—*Weekly Reports of Births and Deaths registered in London from 31st of March to the 15th of September 1849.*  
Published by authority of the Registrar-General.

IN the former part of the present volume, published in July, we expressed the apprehension, that though the epidemic preva-



lence of cholera seemed for the time to have stopped, yet it was impossible to say how soon it might return, or what places, which had not at that time been visited by the disease, might yet feel the weight of its virulence. This opinion was founded on the facts, that, though the disease had apparently ceased for the time in Glasgow and the neighbouring towns, in Dumfries, in various parts of Ayrshire, and in Edinburgh and its vicinity, and though, after being sufficiently severe in several parts of London and the vicinity, it had undergone, during the months of April and May, a great and decided diminution; it began in the last week of May and the first week of June to increase. This apprehension has been realized in most impressive characters. The disease has not only returned in London, but appeared in almost all the considerable towns in England, first in the south, and subsequently further north. Bristol, Plymouth, Devonport, Causand, Portsmouth, along the south coast, and many other places in the centre and on the east coast both of England and Scotland, have been visited more or less severely and generally. At present, however, we propose merely to consider the distemper in London.

During the three last weeks of March, 23 deaths were all that occurred in London from cholera; and this amidst a general mortality of 3372 for the three weeks, or at the average rate of 1124 each week. During the last week of March the mortality was above the average by 72, being 1241. But it is observed in the report, that this is owing, not to any sudden increase of mortality, but to an accumulation of Coroners' cases which occurred on the previous weeks, and which were not registered till the end of the quarter. The average of 1124 is, therefore, probably not far from the truth. It is at the same time observed, that cholera had nearly disappeared from London, the deaths during the last week of March being not more than four. Diarrhoea had also declined.

This satisfactory state of the sanitary movements of the metropolis continued during the whole of the months of April and May, a space of rather more than eight weeks, indeed, sixty-one days. During this time, the whole deaths from cholera amounted only to 15, or 1 death in the course of four days. The general mortality during these eight weeks was 8028, or 1007 each week. The principal mortality was from consumption and diseases of the lungs in the first part of these periods, and pneumonia and bronchitis in the latter part. The last week of May was, however, the termination of this salubrious condition.

During the fourth week of May, 5 deaths had taken place by this disease, and in the week ending May and beginning June, 9 fatal cases had occurred. This increase, though trifling, was, nevertheless, the commencement of a new and rapidly-augmenting mortality.

During the ensuing four weeks, which terminated the month of June, the deaths by cholera were respectively 22, 42, 49, and 124. Of the 42 who died during the second week of June, 30 died on the Surrey side of the river, and the majority of these in Leather Market, Bermondsey. Of the 49 deaths in the third week, 11 took place in persons under 15 years, 32 between that age and 60, and 6 in persons at and above 60 years. These cases occurred mostly on the Surrey side of the Thames, and in the eastern districts on the Middlesex side;—Bermondsey Leather Market still preserving its bad pre-eminence. Of the 124 deaths in the last week of June, 76 were in males, and 48 in females; 21 between 5 and 15 years, 88 between 15 and 60, and 10 at and above 60. The most of these cases were furnished by the central, the east, and the south districts. The cases were generally distributed; but 10 deaths took place in Milbank Penitentiary, Westminster.

From this period, the disease rapidly and steadily increased, and though many cases occurred on the north side of the river, either in the city or the eastern districts, by far the greater proportion took place on the Surrey side, at Rotherhythe, in the borough of Southwark, and in Lambeth. The numbers for the four weeks of July are respectively 152, 339, 678, 783. Among the 152 cut off in the first week, 88, more than one-half, were between 15 and 60 years; 94 were males, and 58 were females;—24 were in Rotherhythe, 13 in Bermondsey, and 16 in Lambeth.

In the second week of July, the disease takes a remarkable and sudden increase, causing considerably more than double the number of deaths of the previous week. Of this number of 339, 192 were in persons between 15 and 60; and, though the disease was still most prevalent and fatal on the south side of the river, where it proved fatal to 192 persons, yet it was increasing generally over the whole metropolitan districts, especially those not remote from the Thames.

During the third week of July, the mortality by cholera rose to exactly double that of the second week. Among the number of deaths, 678, this week, 443 were on the south side of the Thames; 53 in Newington; 51 in St George's, Southwark; 64 in Bermondsey; 106 in Lambeth; and 37 in Rotherhythe. The deaths by diarrhoea were 131,—another circumstance indicating the strong epidemic prevalence. The general mortality had now attained the amount of 1741; a number which exceeds the average of the season by 733. This shows distinctly enough that this large increase in the weekly mortality is principally, if not solely, caused by cholera and diarrhoea, or by gastro-enteric distemper.

The only remarkable circumstances in the fourth week of July are the steady increase in the mortality, both general and choleric,

and the large prevalence of the disease on the south side of the Thames. The general mortality is 1931,—a number nearly double the weekly average. The mortality by cholera is 783, and that by diarrhœa 224. On the Surrey side, the deaths were, in Bermondsey, 64; Newington, 66; St George's, Southwark, 70; and Lambeth, 111,—altogether, 331, not much under one-half of the whole deaths by cholera. In other parts on the Surrey side, were 183, making altogether 514 deaths. At the same time, it must be observed, that considerable numbers of deaths occurred on the north side of the river, in the West London district, the City, Shoreditch, Whitechapel, St George's-in-the-East, and Poplar.

The last three days of July, and the thirty-one days of August, formed a space of nearly five weeks; and during the whole of these five weeks, excepting the second, the mortality by cholera underwent a steady and very large augmentation, until, during the last week of the month, the total deaths by cholera amounted to 1663; and if to these be added 234 deaths by diarrhœa, the number amounts to 1897, on the borders of 1900 persons in one week by gastro-intestinal disorder. Among this number of 1663, 974 took place in persons between the ages of 15 and 60.

During these five weeks, there died by cholera alone, 5914 persons, or 1183 on an average every week. This number is considerably above the average weekly mortality of London by all diseases; and the fact that it is higher than this average weekly mortality, shows as clearly as can be the destroying powers of the disease.

By far the largest proportion of the mortality continued to take place in the densely-inhabited districts on the Surrey side of the Thames; namely, Rotherhythe; Bermondsey; St George's, Southwark; Newington; Lambeth; and Wandsworth.

During the second week of August, or that ending on the 11th, a great sensation was excited by the circumstance of 17 deaths taking place in the course of two weeks in ten houses at Albion Terrace, Wandsworth (eventually 19 persons had died in this place); and not a little controversy has since arisen as to the exact cause of the fatality of the disease in this locality. By some it was ascribed to the drains and cess-pools overflowing in consequence of rain, and the water getting into the wells which supplied the inhabitants with water for culinary and domestic purposes. Others again ascribed it to the disturbance and removal of rubbish from one of the neighbouring houses. And some ascribed it to the disturbance of the contents of a drain or drains and sewers by flushing. It is possible that the disturbance of filth, which, when left in a state of rest and quietude, is quite peaceable and harmless, may give rise to the extrication of foul vapour, miasmatic

air, and all sorts of pernicious exhalations. Yet it is overlooked that these same circumstances are perpetual and constant, while cholera appears only once in a long course of years; and it is not sufficiently attended to that cholera, though certainly very prevalent where drains are obstructed and their contents are allowed to contaminate the atmosphere, takes place in various localities, where no evils of this kind can be detected.

In many parts of these returns it is observed that the houses are good, well-ventilated, and provided with good drains.

In the Report of the Registrar for the third week it is said, "It," that is, cholera, "has now crossed the river." This statement is not very intelligible, and is calculated to confuse the reader. The disease was at no time confined to one side of the river; for, though it was extensive and violent all along the south bank of the Thames, from Lambeth to Rotherhithe, it was prevailing, we have seen, on the north bank in Whitechapel, Bethnal Green, Shadwell, Poplar, and other places.

In the last week of this month, August, the total mortality by all diseases had risen to 2796, of which 1663 were by cholera and 234 by diarrhoea, as already stated. The mortality exceeds that of any previous week since the registration was commenced. The greatest number registered in any single week since 1840 was in the week ending 4th December 1847, when 2454 deaths were registered during the last epidemic prevalence of influenza. In the cholera epidemic of 1832 the parish clerks returned in the old bills of mortality 1021 funerals for the week ending 28th of August; which, allowing for defects in returns and for increase in population, are equivalent to 2450 deaths at the present time.

At this period it is not easy to say in which parts of the metropolis the disease was most prevalent and most rapidly destructive. Bethnal Green, one of the north-eastern districts, is specified as one in which great consternation prevailed, in consequence of the great number of attacks and the great number of deaths; and 128 (144) deaths appear by the reports to have taken place in this district. Yet others are not less inauspiciously pre-eminent. Thus, Westminster, which gives 75; London City and East and West London, 100; Shoreditch, 139; Whitechapel, 74; and Stepney, 64, are all remarkable for their large mortality on the north side of the river; and on the Surrey side we again meet with Bermondsey; St George's, Southwark; St Mary, Newington; and Lambeth, the eleven districts of which present a large list of deaths,—180 at least. Lambeth, indeed, and Westminster, are manifestly the places most fatally afflicted by this distemper; and in this we see nothing very remarkable. Both occupy a low site, stretching along the banks of a long reach of the Thames, in many points below the level of the river; both are exposed to the percolation from the river

through a spongy porous soil ; both are exposed to the exhalations which arise at low water from extensive surfaces of exposed foul mud ; and if, upon such physical and topographical peculiarities, atmospheric agency operates unfavourably, it is not difficult to see that disease is likely to spring up. These parts have also, at all times, been remarkable for the prevalence of diarrhoea and dysentery.

The disease continued on the increase during the first week of September. In that week, in which the general mortality had attained the large figure of 3183 human beings, cholera destroyed 2026, or fully two-thirds. Diarrhoea was little increased, being not more than 272. Of this large number of 3183, 1741 were registered as having taken place on the north side of the river, and 1442 on the south side. This is a diminution as to the former by 19 deaths below the previous week, but an increase as to the Surrey side. The decrease in the numbers on the north side was in the districts of Westminster, Bethnal Green, Shoreditch, Whitechapel, and Stepney. The increase on the Surrey side was in the usual localities,—Lambeth ; Bermondsey ; St George's, Southwark ; Wandsworth ; Camberwell ; and Rotherhythe. In the course of the week, 279 persons died in Lambeth alone. It is stated, on the authority of the registrar, that, of these 279 cases, at least half the cases were allowed to proceed unchecked, until the most dangerous symptoms had manifested their presence.

During the second week of September, ending on the 15th, the total mortality amounted to 2865 ; of which 1682 were by cholera. A diminution has taken place.

From the first commencement of the epidemic, dated from the 23d September 1848, up to the 15th of September 1849, the number of persons which it has destroyed amounts to 12,837. Of these deaths, 6648, or fully one-half, occurred on the south side of the Thames.

To pursue this subject further at present is premature. Much speculation and inquiry have been employed to discover the causes of the disease recurring and producing so great mortality. The great and universal mode of explaining these circumstances is still drains, cess-pools, sewers, all sorts of filth, foul water, and want of ventilation. It is needless to say, that all these may be causes of contamination of the atmosphere, must weaken and impair the strength of the inhabitants, and compel them, accordingly, instead of having recourse to fresh, pure, dry air, to swallow large quantities of what they consider cordials and support, by which are understood every species of poisonous food and drink. Whether these are actually causes of cholera, remains still a question open to further evidence.

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## PART III.

### MEDICAL INTELLIGENCE.

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#### PHYSIOLOGY.

1. *On the Origin of Sugar in the Animal Economy.* By M. Dr Cl. BERNARD, P. of A. and Experimental Physiology. (Archives Generales, Nov. 1848).—In the vegetable kingdom sugar is profusely distributed, and it is found also in animals. Plants cannot find it prepared in the soil, and it is evident that it is formed in their organs. Is it the same in animals? or is the sugar found in their bodies furnished exclusively by saccharine and amylaceous plants which they use as food? This question, which has long occupied the attention of physiologists, M. Cl. Bernard, endeavours to determine experimentally.

As an alimentary article, sugar is a substance which is consumed by man and animals in different conditions. The sugars which are habitually introduced into the alimentary canal are the following:—1st, Cane sugar, or sugar of the first sort; which is met with in the sugar-cane, beet-root, carrot, parsnip, and similar vegetables. 2d, Grape sugar, or sugar of the second sort, which exists in the grape and the saccharine fruits. *Fecula*, or starch, which constitutes a very abundant alimentary substance, may be classed with the sugars, because, in consequence of digestive processes, it is converted in the alimentary canal into sugar of the second sort. Hence, bread and the preparations of flour and meal, as well as potatoes, may be referred to this head. 3d, Sugar of milk, which makes part of the milk of animals.

It is allowed that, as certain aliments are capable of furnishing considerable quantities of saccharine matter, these may be considered as the sole source from which proceeds the sugar found in the blood and in the fluids of animals. It is also allowed, that sugar exists in the blood in animals, only under the condition, that these have previously eaten substances which contain it, or are capable of producing it. On the one hand, chemical facts show, that among alimentary articles starch alone can be converted into sugar; and, on the other hand, connecting this question with the idea, that animals create no proximate principle, and merely destroy those furnished to them by the vegetable kingdom, physiologists have believed themselves justified in denying most explicitly to the animal economy the faculty of preparing sugar, and allowing to it only the faculty of destroying it and causing its disappearance. To this conclusion the facts contained in the Memoir of M. Cl. Bernard are opposed. †

It is observed that, during the digestion of saccharine or amylaceous food, the blood of man and animals contains sugar; and from this it was inferred, that sugar is furnished by alimentary substances.

Four series of experiments were performed and many times repeated. From the first series, in which M. Bernard fed animals with vegetable articles containing sugar, and animal substances void of that article, and also kept them in a state of abstinence from food, he finds that there is constantly sugar in the blood of animals, whatever be their food, and even in the state of abstinence.

The next object of inquiry was to ascertain the source of this sugar,

and the organ or organs by which it is manufactured. He found first, that when a dog is put to death after several days of abstinence, the blood of the portal vein and of the right ventricle contained sugar; while none was found within the thoracic duct. He found next, that in a dog killed during the digestion of matters void of starch and sugar, yet there is sugar in the intestinal veins and in the matters contained in the intestines; that the blood proceeding from the spleen contains no traces of sugar; that the blood of the pancreatic veins, is equally void of that substance; that in the blood of the hepatic veins divided after a ligature had been placed on the portal vein, large quantities of sugar were present; and that the substance of the liver furnished sugar, while those of the spleen, pancreas, and mesenteric glands furnished none.

M. Bernard often examined carefully the substance of the liver, in order to ascertain the best mode of proving the presence of sugar in this gland, and to determine the nature of the sugar. He finds, that a bit of liver reduced to minute fragments furnishes it readily, and that the substance of the liver itself, if tasted, putting aside the bitter taste of the bile, is sensibly saccharine. This sugar is neither sugar of milk nor cane sugar. It is grape sugar or glycose.

M. Bernard further ascertained, that in animals in which the pneumogastric nerves were divided, while the process of digestion was advancing, neither in the blood nor in the liver was the presence of sugar detected. *Lastly*, He found that in young calves brought to the slaughter-houses, sugar exists in the liver in great quantity; and he infers that it is produced during intra-uterine life.

The general results, from all these experiments, are the following:—

1. Diabetic sugar exists constantly and normally in the blood of the heart and in the liver in man and animals.

2. The formation of sugar takes place in the liver, and it is independent of the use of saccharine and amylaceous diet.

3. This formation of sugar in the liver is commenced in the animal before birth, consequently, previous to the direct introduction of alimentary articles.

4. This production of saccharine matter, as one of the functions of the liver, appears to be connected with the integrity of the pneumogastric nerves.

From these facts, it results that the law according to which it is taught, that animals generate no proximate principle, but only destroy those submitted to them by animals, cannot be well founded, since animals, in the physiological state, may, as well as vegetables, produce and destroy sugar.

M. Bernard allows, nevertheless, that it does not follow, that in every dead body we are certainly to find sugar in the liver. There are many diseases, he thinks, in which the sugar disappears, and is not found in the liver. Sugar, it is observed, disappears from the urine in diabetic patients in the last period of life; and M. Bernard adds, that it also disappears in these patients from the liver. In the bodies of 18 persons dead of different diseases, he searched for sugar in the liver. Some furnished different proportions of it; others gave no trace. In animals enfeebled by long abstinence, diseased, or dead by disease, sugar is often diminished to a great extreme, and may completely disappear. All the livers of the animals brought to the shambles ought to contain a good deal of sugar, if they have been slaughtered in proper condition. Livers found at the tripe-dealers' always presented a large amount of sugar.

An important point is, to ascertain whether sugar exists in the same proportion in all classes of animals, taken in conditions as similar as possible. M. Bernard finds, that, *1st*, in the common fowl and pigeon the

proportion of sugar is very considerable; 2*d*, in the dog, rabbit, hog, ox, calf, and horse, the proportion of sugar is likewise considerable; 3*d*, in the frog and in the lizard the quantity of sugar found in the liver is very small; and 4*th*, among fishes, in the skate and eel, though examined as recently as possible, he found no trace of sugar.

This progressive disappearance of sugar in cold-blooded animals is a curious phenomenon which deserves attention, if not inquiry.

#### MATERIA MEDICA.

1. *On the Extraction of Mannite from the Dandelion.* By MESSRS SMITH; with an Analysis of the Mannite, by Dr Stenhouse. (Proceedings of the Royal Society of Edinburgh, Vol. ii. No. 34, 1849.)—Messrs Smith state that they have extracted from the dandelion a large amount of a crystalline sweet substance, having all the physical characters of mannite. It was analysed by Dr Stenhouse, and found to contain carbon, hydrogen, and oxygen, in the proportions which characterise the accepted formula for mannite; viz.,  $C H, O_6$ , so that it certainly was the substance it was supposed to be.

Messrs Widmann and Frickhinger, it was stated, had anticipated Messrs Smith in the separation of mannite from the dandelion juice, and were led to believe that the mannite did not pre-exist ready formed in the dandelion, but was formed in the juice as the result of a peculiar fermentation which it underwent. This result was confirmed by the Messrs Smith, who experimented with large parcels of the plant, and found that even from quantities of the fresh root, so large as 40 lb., no mannite could be extracted, if the expressed juice were prevented from fermenting; whilst, if fermentation were permitted, the same weight of roots yielded a large quantity of mannite, which appears to be derived from the sugar, inulin, &c., of the dandelion, which were converted into mannite, gum, and lactic acid.

The Messrs Smith state, in conclusion, that they have not been able to confirm the statement of Poxley, that the dandelion contains a bitter crystallizable substance, such as he had described under the name of Taraxacine.

2. *On Lerp, a new species of Manna from New South Wales.* By THOMAS ANDERSON, M. D. (Proceedings of the Royal Society of Edinburgh, Vol. ii. No. 34, 1849.)—About thirty years ago, a species of manna, obtained from the *Eucalyptus Mannifera*, was brought from New South Wales, and was examined by Dr Thomas Thomson, and afterwards by Professor Johnston, both of whom ascertained it to contain a new species of sugar, different from the mannite which exists in ordinary manna. The author had, through the kindness of Mr Sheriff Cay, an opportunity of examining a very different species of manna, remarkable both from its chemical constitution, and from its possessing a definitely organised structure. This substance was discovered by Mr Robert Cay in 1844, in the interior of Australia Felix, to the north and north-west of Melbourne, where it occurs at certain seasons on the leaves of the Mallee plant, (*Eucalyptus Dumosa*), and is known to the natives by the name of Lerp.

It consists of numerous small conical cups of the average diameter of a sixth of an inch, more or less distinctly striated, and covered on the outside with hairs of considerable length. The cup resembles some of the smaller species of patella, and its mouth is perfectly smooth and round. Several of the cups are frequently attached to one another by the edges, and always so that their mouths form a plane, by which it would appear they have been attached to the leaves. The hairs, when examined under the microscope, were found to consist of uniform tubes,



with a granular structure, and indistinct traces of transverse striæ; they are coloured uniformly blue by iodine. The cups are made up of a confused mass of closely-compacted cells resembling starch globules, and coloured blue by iodine.

The taste of Lerp is distinctly saccharine, but this is confined entirely to the hair, the cup having merely a mucilaginous taste. The chemical examination showed it to consist of an uncrystallizable sugar, similar in its character to that found in fruits, of starch, gum, inulin, and cellulose, the absolute identity of the latter two of which was determined by ultimate analysis. There were also found minute traces of resinous matter and nitrogen, and 1.13 per cent. of ash. The following is the result of its quantitative analysis:—

Water.....	15.04
Sugar, with a little resinous matter.....	49.06
Gum.....	5.77
Starch.....	4.29
Inulin.....	13.80
Cellulosa.....	12.04
	<hr/>
	100.00
Ash.....	1.13

The author, in concluding his paper, remarks that all the species of manna before observed consisted of soluble substances, and were considered to be produced by the puncture of an insect, which caused the exudation of their constituents in the fluid form, and that they gradually dried up upon the surface of the leaf, but that the existence in Lerp of the insoluble cellulose and starch, and the sparingly soluble inulin, seemed scarcely compatible with such an explanation of its origin.

#### SURGERY.

*On a Local Mode of abating the Pains ensuing on Chirurgical Operations.* By M. JULES ROUX. (Comptes Rendus, 27th Nov. 1848, T. xxvii. p. 553.)—The pains attending surgical operations may be distinguished into those of the operation, those immediately following the operation, and those consecutive to the operation, or which take place during cicatrization. M. Roux proposes a means of alleviating these pains by direct application.

This consists in applying an anæsthetic agent for five, ten, or fifteen minutes, in contact with the wounds. This is effected by forceps, a bit of charpie, a sponge, or a bag, if the liquid is applied in the form of vapour, or by sprinkling the wounds, or filling their cavity with the anæsthetic agent. Hitherto, M. Roux has used only aldehyde, ether, and chloroform; and, to the last, most of his facts relate.

The operations in which he has used the means, are those for phimosis, amputation of the finger, of the thigh, and of the fore-arm.

When patients have not been previously etherized by pulmonary inhalation, they complain of a sensation of pricking, scorching, unpleasant heat, acute pain, and, eventually, burning. A little after, these feelings diminished quickly, disappeared, and yielded to partial etherism, limited to the points directly etherized. This local insensibility lasted for forty-eight hours at least, and left the patients in a state of perfect calm.

When, on the other hand, patients have undergone operations in a state of etherism, and when, before the return of general sensibility, the wounds have been exposed to direct etherization, the pains resulting from the action, at first irritating of the anæsthetic liquids, were not felt, and the patients altogether restored to consciousness, passed without suffering the two or three first days which followed the operation.

Local anæsthesia, produced by direct etherization, lasts a time sufficiently long to annihilate the pains of the second order.

On the 8th of November 1848, M. Roux performed, on a man aged 30 years, amputation of the left fore-arm. After the separation of the limb, and the application of four ligatures, the stump was filled with chloroform, while the patient was still in a state of unconsciousness from inhalation of the chloroform vapour. All the wounded surfaces were exposed to this vapour for the space of about ten minutes, while the patient, who, in this interval, had recovered sensibility, felt no uneasiness at the local action of the chloroform. The stump, without any dressing except a single cloth, was placed on a cushion.

For two days, the patient declared that he felt in the wound not the slightest pain; and after this period, he stated that he felt in it, only at intervals, trifling uneasiness. Though the patient had been wounded also in the right eye and in the right fore-arm, he had no fever; sleep was perfect; appetite good; the progress of the wound regular. On the eleventh day after operation, cicatrization was completed at the angles of the wound, and promised to be very soon finished.

In pains of the second order, local etherization was useful in shortening their duration and abating their intensity.

Pains of the third order are not uniformly so severe as to require the employment of local etherization.

One form of pains of this order, namely, those attending suppurating wounds, were greatly abated, and even extinguished, by the local use of chloroform, which further exerted on the general progress of these surfaces a favourable influence. Inguinal ulcers, the consequence of syphilitic glandular affections, which are wont to be rendered very painful for several hours after the application of lunar caustic, became insensible after the use of this remedy with direct etherization.

It is to be observed that chloroform, though it painfully reddens the skin, covered by epidermis or not, without producing complete asthenæsia, produces, on the other hand, on the surface of wounds, with temporary irritation, subsequent well-marked insensibility. The direct application of this remedy is perfectly innocuous.

Such are the statements of M. Roux. It will not be easy to persuade every one to assent to the opinion, that the use of chloroform is innocuous.

The experience of nine or ten months has furnished a sufficient number and force of proofs to satisfy those, who draw inferences from facts, that the administration of chloroform is not, in any sense of the term, innocuous. That its local application may be less active, and, consequently, less hurtful than its general use by inhalation, may be the case. When inhaled, three conditions are produced. 1. Not only is it applied to a very extensive absorbing membrane, through which it is received by endosmosis into the blood; but, 2, when so received, while it excludes atmospheric air; 3, it paralyzes the muscles of inspiration, and thereby prevents the patient from breathing. These conditions are obviated by the local application. But it is again to be remembered, that, in the local application, the narcotizing vapour is applied to an extensive surface, presenting many nerves and blood-vessels, and, consequently, the agent acts at once on the nerves and the blood of the part. It may be answered that the action is only local; and so it certainly is for some time. But, eventually, the chloroform may be absorbed, and, if not eliminated as other narcotic agents, it may produce effects which are any thing but innocuous.

#### LEGAL MEDICINE.

*Death by Inanition.*—In observations on the example of death by

inanition recorded in the following page, Mr Headland refers to another instance, which is shortly noticed in Taylor's Medical Jurisprudence. The authentic account of that case is here given as introductory to the instance by Mr Headland. In several respects, the similitude of the cases is doubtful. But readers may compare them, and draw their own conclusions.

On October 8, 1835, about five in the morning, part of the Kilgrammie coal-pit, on the estate of Bargany, fell in. The crush continued for two days, so as to render it extremely dangerous to enter the pit. All the colliers escaped except John Brown, an unmarried man, about sixty, who either did not hear the noise of the fall, or disregarded it, till he found himself enclosed in the ruins. Attempts were made to extricate him at different times; but the foul air which had collected in the pit, and other circumstances, prevented their success. The idea of his being alive after a fortnight was scarcely entertained by any one. But the place where Brown had been at work, all continued anxious to reach. Great progress was made in clearing a passage to it on October 30th. Some of the colliers, in the belief that groans had been heard, returned to the work in the course of the night, and after three hours' labour, they found him lying on the ground, still living, and able to recognize them, but so feeble, that the utmost he could say was the first syllable of one of their names. The aperture by which they had entered was so low and small, for a considerable way, that it was only by laying him on a board, and shoving it along, that he could be taken out. A considerable time also had to pass before he could be raised to the pit head, as at three or four in the morning, none were astir but the men who were with him. During the interval, they stripped themselves of their own clothes, to cover him from the air. One had the remains of a piece of bread and butter which he had carried with him to the pit, and with the butter he rubbed the man's lips, which were excessively dry and parched. Another ran to a distance by the level, and raised a neighbour to milk her cow, as Brown had expressed a desire for milk. A third went by the same way to acquaint their master with Brown's being alive, and to procure assistance for bringing him to the ground. The rest lay in succession with their naked backs to his, to produce something like natural heat, in his almost ice-cold body. The news quickly spread of his having been found alive, and visitors innumerable crowded to the cottage where he was. By 9 o'clock, when the writer of these pages saw him, he was comfortably laid on the bed which he had been accustomed to occupy. There was then an agreeable warmth on his skin, and although he spoke slowly and little, yet he was perfectly collected. His appearance was very striking, from the length of his black and glossy beard, the hollowness of his eyes, and their extreme brightness, as if they had been glazed. The odour of his breath was particularly offensive. After answering some questions, it was his own request that prayer should be offered up for him. When the prayer was finished, he grasped the minister's hand, and expressed his thanks. A medical man attended him repeatedly. Instructions were given to those about him, as to the mode in which he should be treated; and for a day or two, it seemed as if there was a possibility of his regaining strength. But on November 2d, he began to sink, and on the evening of the 3d he quietly expired.

The sheriff of the county was ordered to inquire into the circumstances which led him to be so long immured, and also into the state of his body after death. But nothing was elicited to attach blame, either on account of the length of his imprisonment, or on account of the treatment which he received while under medical care.

He had a lamp with him in the pit, and a portion of oil to feed it.

But he was in total darkness during almost the whole time of his imprisonment. The oil he never thought of tasting, and he had no food of any kind. As long as he was able to walk, he drank of the strongly-impregnated water which flowed through the room in which he worked. It was from thirst alone, he said, that he suffered. For about a fortnight he was able to walk to and fro. After that he had not strength to reach the stream which served to allay his thirst. He never lost the hope of being taken out. His mind, therefore, was calm. There was no exciting cause to wear out his existence. He had some notion of the progress of time, from hearing the colliers at work; and he always conceived that they were attempting to reach him. When addressed he could speak only in a feeble whisper. Of his thoughts and feelings he could give no account; and, owing to his enfeebled state, it was impossible to press him with questions on the subject. That he was sensible of the wonderful care which had been exercised over him, and of his very precarious state, may, it is hoped, be inferred from his asking to be recommended to God in prayer.

*Post mortem* examination and medical report on John Brown. Dailly; 13th November 1835.

Brain rather paler and firmer than usual; ventricles nearly empty; choroid plexus pale.

Omentum nearly altogether absorbed, rests merely upon the colon; capable of being extended about two inches.

Heart small and flabby.

Stomach of medium size, empty at the cardial extremity; inflammation of the mucous coat extended about two inches in one direction, by one inch and a-half in the other.

Mucous coat abraded in two places about one quarter of an inch.

Liver healthy; gall-bladder very much distended; no mechanical obstruction.

Spleen very black,—easily ruptured.

Intestines healthy; mucous coat dyed by a dark-coloured fluid.

Bladder, structure healthy,—about half a pint of urine.

Kidneys healthy.

Lungs healthy; lobes loaded with black matter.

The body was extremely emaciated.\*

It may be observed, in reference to the statement that inflammation of the mucous coat of the stomach extended about two inches in one direction, by one inch and a-half in the other, that this was not inflammation. The appearance was merely that of vascular, most likely venous, congestion, an appearance imitating inflammation, but by no means the same. This appearance is not uncommon on the mucous surfaces in general, and has been observed in other instances of death by inanition.

*A Case in which Death was probably the result of Starvation.* By EDWARD HEADLAND, Vice-President of the Medical Society of London. (Trans. of the Medical Society of London. New Series. Vol. i. 1846.) —A solicitor, aged twenty-six years, suffering under what he called a feverish cold, applied for relief to a homœopathic practitioner, who directed that he should live entirely on water, until he received further instructions; this the patient strictly obeyed, and continued from day to day under the direction of his adviser, until ten days had been passed with nothing but water for his support. On this day, the nurse who was in attendance becoming alarmed at his condition, besought his attendant to give him food, and, after the matter had been much pressed, he consented to his having *one teaspoonful of beef tea*. The nurse

\* New Statistical Account, Vol. v., Ayrshire, p. 392, parish of Dailly.

gave him two, and two more on the following morning; so that this being the twelfth day, he had had water and four teaspoonfuls of beef tea only during the whole time.

It was on this day that Mr H., in company with a well known and distinguished physician, saw him for the first time. He was lying on his back with his legs extended, very much emaciated, and not a particle of fat could be observed or felt in any part of the coverings of the body; the abdominal muscles were drawn back upon the empty bowels close to the spine, forming a concavity (bounded by the lower ribs above, and the anterior bones of the pelvis below) so deep that it would have contained some pints of fluid. There was but very slight uneasiness when pressure was made upon these parts. The eyes were sunken and glazed, the vessels of the sclerotic redder than usual, and the pupils slightly contracted. The body generally was of the natural temperature, the feet rather cold, the tongue clean, and presenting no deviation from its healthy state. He required the mouth to be constantly moistened, for which purpose he used a rag soaked in water. The urine was constantly escaping, his pulse was 82, and feeble. The most prominent circumstance was the loss of a large quantity of dark-coloured grumous blood from the bowels, resembling what is met with in sea scurvy, having a great deficiency of fibrine. This began on the previous day. He had constant hiccup, was restless, and complained of nausea. He was without delirium.

The history received of his case, with a slight consideration of his symptoms, made it obvious that the only chance of his surviving was by the careful administration of nourishment. Beef tea and arrowroot were directed to be given in small quantities alternately; and, if there should be further sinking, the careful addition of wine or brandy, and the use of effervescent drinks was recommended. Vomiting occurred soon after the first food was administered, and, notwithstanding all the means used, continued to nearly the time of his death, as did the bleeding from the rectum. Death occurred on the thirteenth day.

It was thought desirable that the *post mortem* examination, under the circumstances, should be made by a party who had not been engaged in his medical treatment, and Mr Hancock kindly undertook to conduct it. The emaciation already referred to was very striking. In the course of the examination fat was nowhere met with. The intestines occupied but a small space. The stomach contained a small quantity of beef tea, which had been administered a short time before death; it exhibited nothing worthy of notice beyond a very slight redness upon its greater curvature. The liver was of its natural size and texture, giving no mark of disease; the gall bladder greatly distended, with thick bile, of the colour of sap green, which colour was imparted to the covering of the duodenum, and all the parts with which it came in contact. The spleen, pancreas, and kidneys, perfectly healthy. The intestines, with the exception of some grumous blood in the rectum, were empty throughout. The duodenum and jejunum were without mark of disease, but perhaps slightly redder upon their inner surface. About seven small ulcers were discovered scattered upon the inner surface of the ileum and cæcum, and both these intestines considerably congested. Several spots might be observed, with small vessels greatly elevated, and seemingly upon the point of giving way, and degenerating into ulceration. There was an entire absence of lymph, and of all marks of increased action, the state being one of simple congestion, as distinguished from inflammation. There was also an intussusception of the ileum, a simple dipping in of about four inches of the intestine, without the slightest redness or other evidence of action; the bladder was slightly ecchymosed upon its inner surface.

The lungs throughout were without disease, with the exception of a few very small tubercles in a quiescent state in the neighbourhood of the bronchi. The heart was also without any mark of disease, and nearly without blood, as was the body generally.

The membranes of the brain were healthy, with the exception of a very slight effusion between the arachnoid and *pia mater*. The brain was firm and healthy, and, as far as the eye could detect, under very careful examination, free from disease of every kind. The vessels, however, in every part were gorged with blood, as though the last blood in the body had concentrated itself in this organ.

The question which the circumstances in this case suggest is this,—did the condition of the intestines, which preceded the ulceration from which the hemorrhage occurred, arise from the long abstinence of the patient, or was it a condition of intestine so well known to exist in fever? and which has been so ably delineated in some papers by the late Dr Armstrong; a condition, however, with which everybody is familiar. The ulceration now mentioned is common in fever; but it was unattended by any of the usual circumstances. In fever, under these circumstances, the tongue is invariably either brown and dry, or preternaturally red; the pulse invariably quick. In this case the pulse was but 82, and the tongue in no way deviating from health. Mr H. never knew a case of fever with ulceration where the pulse was below 100, and generally much beyond. There is, too, always tenderness upon pressure in fever,—here there was none, or scarcely any. The disorder was not ushered in by the attendant circumstances of fever of this intensity; the patient had no want of appetite in the onset, or through the greater part of his disorder; it was that he was not allowed to eat, not that he could not. The want of blood in all parts of the body, except the brain, was opposed to fever of this character. The existence of the large quantity of blood in the brain doubtless immediately preceded death only, its existence at an earlier period must have been accompanied by cerebral symptoms, from which he was entirely free; and there is no difficulty, Mr H. thinks, in explaining its condition in another way.

MISCELLANEOUS.

*Medical Graduations at Edinburgh.*—On the 1st of August 1849, the Senatus Academicus of the University of Edinburgh conferred the Degree of Doctor of Medicine on the following gentlemen, in number fifty-one, after having gone through the appointed examinations, and defended publicly their Inaugural Dissertations.

FROM SCOTLAND.

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|---|--|
| James Brewster Balfour, on Epidemic Cholera.  | William Maclean Mackenzie, on Induced Anæsthesia.                    |
| Alexander Brown, on Scarlatina.   | William Reid, on Spinal Paralysis.                                   |
| James Cameron, on the Nature and Consequences of those Injuries to which the Head is liable from External Violence. | Alexander Adam Renton, on Muscular and Chronic Articular Rheumatism. |
| John Edw. Cathcart, on Tetanus.   | John Hutchinson Robertson, on Asiatic Cholera.                       |
| Robert Wilkie Crichton, on the Alimentary Functions.  | William Sanders Rutherford, on the Anatomy of the Spleen.            |
| Lockhart Ross Frame, on Tetanus.  | Alexander Scott, on Phthisis Pulmonalis.                             |
| James Grant, on the Causes which influence the Position of the Fœtus in the Uterus.                                 | Andrew Heatley Thomson, on British Continued Fever.                  |
| William Johnston, on Acute Hydrocephalus.   | William Warden, on the Inhaling of Gases and Vapours.                |
| William Johnstone, on Pneumonia.  | James Atkinson Wilson, on Menstruation.                              |

Wm. Ziegler, on Inflammation of the Intra-Pelvic Cellular Tissue.

FROM ENGLAND.

Archibald Leslie Archer, on the Urine as a Sign of Disease.

Robert Barnes, on the Purposes of Excretion.

Francis John De Quincey, on the Religious Objections to the Use of Chloroform in Obstetric Medicine.

William Murray Dobie, on Voluntary Muscular Fibre.

Arthur S. Donkin, on Anæsthesia.

Charles W. I. Moffat, on Scarlatina.

Henry Nuttall, on the Necessity of Scientific Principles in the Investigation and Treatment of Disease.

William Charles Owen, on Hereditary Predisposition.

Thomas Pearse, on the Physiology and Pathology of the Blood.

Richard Picken, on Necrosis.

Godfred Sandwith, on the Pathology and Treatment of Dropsy.

Henry Edwin Sargent, on Design, as illustrated by the Protective Apparatus of the Arteries.

William Scott, on the Nature, Symptoms, Causes, and Treatment of Gout.

Alfred Sharpe, on the Nature, Functions, and Diseases of the Mucous Membranes.

John Grant Wilson, on the Morbid condition of Urine.

John Wright, on Bronchocele.

FROM IRELAND.

Richard Barnett, on Apoplexy.

Robert Warren Bleazby, on the Pathology, Causes, and Treatment of Hæmorrhage, as it concerns the Physician.

William J. Cummins, on Scurvy.

John Phillips Cunningham, on Intermittent Fever.

Thomas Cunningham, on the Exploring Needle.

Walter Lindesay Richardson, on Bronchitis.

James Sherlock, on the Climate of India.

John Sloane, on Gunshot Wounds.

Robert Whyte, on some Pathological Conditions of the Testicle.

FROM WALES.

Benjamin Davies, on Anæmia.

David Jones Whitty, on Apoplexy.

FROM NEW BRUNSWICK.

Titus William Knapp, Concerning the Action of Remedies.

FROM THE EAST INDIES.

John Chisholm, on Menstruation.

Frederick Corbyn, on Dysentery.

FROM BERMUDA.

Frederick Augustus Stuart Hunter, on the Nature and Treatment of Albuminuria.

FROM THE CAPE OF GOOD HOPE.

Philip John Vander Ryl, Clinical Pathological Observations in Surgery.

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